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JOURNAL

OF THE

SOCIETY OF

Telegraph Engineers and of Electricians.

VOL. XII. 1883. No. 46.

The One Hundred and Seventeenth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 18th, 1883—Lieut.-Col. C. E. WEBBER, R.E., C.B., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The following Associate was announced as having been transferred to the class of Members:—

George Annesley Grindle.

The names of new candidates were announced and suspended, and the Secretary announced that donations had been received for the Library as follows:—The French Government, the Italian Government, Herr Otto Brandes, Prof. W. J. Rankine, Mr. J. E. Salway, and Dr. K. E. Zetzsche (foreign member).

Sir CHARLES T. BRIGHT: Mr. President and gentlemen,—It is a melancholy task to have to speak of the death of a Past-President of our Society, as well as one of my oldest friends—I mean Mr. Charles V. Walker, whose name is known to most of those present. He left us at the close of the past year. My recollection of Mr. Walker dates back from so long a time ago as 1847. His was, perhaps, one of the earliest names known connected with the practical application of electricity to telegraphy. He was not so well known as he ought to have been, on account of a remarkable modesty of character which all those who were acquainted with him (and all those who were acquainted with him loved him) knew better than the public, who were not aware of his great qualifications. Many years ago Mr. C. V. Walker was one of the founders of the Electrical Society, and, as the hon. secretary, was thereafter one of its leading supports. The journals of that Society (which are not possessed by many), edited by him, will show what efforts were made by Mr. Walker and those who aided him (such as Mr. Gassiot, and other names which at so short a notice I cannot call to mind) in the advancement of the science which this Society has to do with, and which indeed is our *raison d'être*. Mr. Walker was for many years the Telegraph Superintendent of the South-Eastern Railway, which, though it was his chief occupation so far as his business was concerned, by no means expresses the services which he rendered to electricity, nor did it even interfere with his constant work at experimental development of the branch of physics which he loved. He was always a most ardent student of the sciences. In the early days he was consulted about many things concerning which some present may perchance know very little. For example, when the first Atlantic cable was laid in 1858, under my charge as engineer, it unfortunately failed afterwards in the electrical department, through lack of insulation, because at that time so little comparative experience and progress in the manufacture had been made with the insulator. Mr. Walker was invited by the Atlantic Telegraph Company, among others, to report on the matter, which he did, and I have a copy of that scarce and valuable document in my possession, for his suggestions were of very great utility. Again, and to make a long story short (though I wish our time would allow me to dwell longer over our late friend's life), I met Mr. Walker in Ireland, on the occasion of the transference of the different Telegraph Companies' lines and stations to the Post Office. Mr. Walker was Commissioner for the Government, while I was engineer to the Magnetic Telegraph Company, and we had much discussion, but I am happy to say very few, if any differences, in order to arrive at a fair valuation of the lines to be handed over. Another historical fact will interest any of those present who are concerned in sea telegraphy. It is within my recollection that Mr. Walker, on behalf of the South-Eastern Railway Company, took out, on board a tug steamer, the very first insulated wire to be laid under the sea as an experiment (which was about two miles in length from Dover), and spoke back to the shore: this was, I think, in 1847, while the first really workable cable, as we know, was laid in 1851. I am quite sure that we shall all unite in the warmest, and, at the same time, most sympathetic feelings in a tribute of regret at the recollection of the death of one of the earliest and oldest members of our Society, and, moreover, a Past-President of the Society, and I beg to move a vote of condolence and sympathy with the family of Mr. Walker, conveying the very deep regret of the Society at the loss they and we have sustained in his death.

Mr. **Willoughby Smith**: I should like to be allowed to second the proposition of Sir Charles Bright. Having known Mr. Walker in the ordinary course of business for thirty years, I can endorse all that Sir Charles has said on the subject.

Mr. **James Radcliffe**, as one of the oldest telegraph engineers, supported the motion. He had been connected with telegraphy for very many years, and had known Mr. Walker longer than he cared to recollect, and on behalf of the Railway Telegraph Superintendents, whose comrade Mr. Walker was, he begged to endorse all the kind remarks which had been made of him.

The **President**: It has been duly proposed and seconded that the condolences of the Society be offered to the family of Mr. C. V. Walker, F.R.S., lately President of this Society, and also that a letter conveying the expression of the approval of this meeting be written to them by our Secretary, as well as an intimation that the Railway Telegraph Superintendents wish especially to accord their sympathy and condolence.

The motion was appropriately carried.

The **President**: Gentlemen,—It now becomes my duty, with great regret, to resign the seat in which you did me the honour to place me last year, as your President. I can only tell you that it is a seat in which one is to a certain extent, on certain days and certain evenings, very busily occupied. But that occupation is of the most enjoyable character, because one is associated with, and aided by, a Council, to which there is hardly (particularly as regards the matter of attendance) a second in this great city.

But my regret is entirely swept away when I know that I am to be succeeded in this chair by one who has been known to electricians in this country a far longer period than I have been, and in introducing him to you I feel that I am only presenting to you an old friend.

The President-elect, Mr. Willoughby Smith, then assumed the chair.

Mr. H. C. **Forde**: Mr. President,—I have been asked this evening, and it is a great pleasure to me to comply, to propose a vote of thanks to Lieut.-Col. Webber for his conduct as President during the past year. I am quite sure that every member of this Society who has been present at the meetings during the past year will cordially join in this vote. As a junior member of the Council, I know well how he has performed his duties in the council chamber; and we all know that he has also had in the meantime to perform very important military duties abroad; but those military duties certainly have not interfered in any way with his duties as President of this Society, and I do not think we could have made a better selection than we did last year. I therefore ask you, gentlemen, to pass the vote proposed, with acclamation.

Mr. C. E. **Spagnoletti**: I have very great pleasure indeed in seconding the proposed vote of thanks to Lieut.-Col. Webber on his retirement from the presidential chair. I have had the pleasure of working with Col. Webber for a considerable time, not only in the Council of this Society, but in duties connected with the Society at home and abroad, and I can bear strongest testimony in every way to the correctness of the remarks Mr. Forde has just made. Col. Webber does his work in a most energetic manner; and when one remembers the military duties he has had to attend to concurrently with those connected with the Society, one is surprised at the complete manner in which he has carried them out. He is most indefatigable at his work, and evidently he loves it, for he does it with such heart and spirit, and sets an example to us all in looking after the interests of the Society, of which he was one of the founders, and which I am sure all his successors will do well to follow.

The vote was carried with acclamation.

Lieut.-Col. C. E. WEBBER, R.E., C.B.: Mr. Chairman and gentlemen,—At such a moment, when we are all expecting the address our President is about to give us, brevity is my motto. If I am energetic, if I love hard work, I can only say I hope that through all my life some of my energy and hard work will be at the service of this Society, in which I always take and have taken the deepest interest, and for which I think every one will say I am justified in foreseeing a great future.

The President then read the following inaugural address:—

INAUGURAL ADDRESS.

By WILLOUGHBY SMITH, President.

Gentlemen,—My first duty this evening is to thank you for the honour you have conferred upon me in electing me your President for the ensuing year. Knowing as I do the illustrious men who have preceded me as Presidents of this Society, I should not have presumed to accept so exalted a position had I not felt confidence in your kind indulgence, and in the assurance that the Council would extend to me that kind assistance which they have ever been ready to render to my predecessors.

This is but a young Society, and necessarily so, for the branch of science in which we have enrolled ourselves as members is a very young shoot in comparison with many of the other branches of the same tree. Telegraph engineers and electricians have no great masters of antiquity to imitate or revere; most of their great men are of the present age. Since the formation of this Society, eleven presidential addresses have been given by such men, who have so thoroughly covered all the ground, and I might add the bottom of the sea also, that I feel a great difficulty in delivering to you the usual inaugural address, which is always looked forward to with so much interest by the members of this and kindred societies; for rapid as has been, and still is, the adaptation of electrical science to every-day life, were I to confine my remarks solely to the new matter which has been introduced since you heard the able address of the gentleman who has just vacated the position I now occupy, I am sure it would be as disappointing to you as it would be unsatisfactory to me. Still it is not my intention to ask you to accompany me to speculative heights or theoretical depths, but rather to walk leisurely along the well-known high-road, thereby contenting ourselves within the limits of real knowledge, rather choosing to confess our ignorance than to give to mere opinions the authority of facts, and by so doing I think sufficient fruit may still be gleaned to produce a supply of entertainment, if not of instruction.

Each branch of natural philosophy has progressed in proportion to the advancement made from time to time in the accuracy of the instruments and appliances required for observations or measurements: no branch has benefited more in that respect than that in which we are individually and collectively so immediately interested. It is probably within the memory of the youngest member of this Society that electricians found it difficult to correctly convey their meaning for want of a recognised system of units. How long we should have remained in such an unsatisfactory state it is difficult to say, had not the British Association for the Advancement of Science, in 1860, appointed a committee to determine the best unit of electrical resistance. It was fortunate that this committee was constituted of eminent men who could so thoroughly realise the importance of the work entrusted to them, and who, after eight years of careful labour, produced, not only a practical unit of electrical resistance, but a coherent system of electrical measurements. It was, however, foreshadowed at the time that a future might arrive when, owing to the progress of the exactness of physical measurements and the effects of time, new determinations might become necessary. There can be no doubt that the time has long since arrived, and that the whole subject requires a careful re-investigation. At the present time we are fast returning to the position we had before the formation of the committee referred to. It has been for some time, and still is, possible to purchase for commercial purposes copper of a higher specific conductivity than that of the recognised standard for pure copper.

It is not necessary to dwell upon the advantages that would accrue from the adoption of a uniform and complete system of electrical units for all the civilised parts of the world, for it is granted on all sides that such uniformity would be desirable, and therefore it was but natural that the proceedings of the committee appointed at the Electrical Congress held in Paris in September, 1881, to consider the whole matter, and to definitely settle an international and permanent system, should have been watched with so much interest. The time, however, at the disposal of the committee was limited, and taking into consideration the place in which the meetings were held, and the various other attractions of the Exhibition, it is not surprising that but little progress was made on the subject beyond a few simple resolutions which were promptly and unanimously adopted. The third of those resolutions was that "the unit resistance (ohm) be represented by a column of mercury having a square millimètre of section at the temperature of zero degree centigrade." Before the appointment of the committee by the British Association in 1860, this unit was known as Siemens' mercury unit, and was fast being recognised, but the committee rejected it on the ground that it was arbitrary, varying, of no practical advantage, and formed part of no coherent system of general electrical measurements. Allowing the weber to be rechristened ampère was no doubt due to a spirit of conciliation which could do no harm; but if this sudden change of the character of the unit were done in the same spirit, it might lead to inconvenience hereafter. To the practical electrician the material standard, not the definition, is the important point; consequently the next proposition, "That an international commission be entrusted with the ascertaining, by a new set of experiments, for practical purposes, the length of column of mercury, having a section of one square millimètre at the temperature zero degree centigrade, which shall represent the value of the ohm," was of more interest. The members of that commission met in Paris early in November last, and, after passing several resolutions, adjourned for twelve months. The adjournment is, I presume, to allow time for desired experiments, so that when the members reassemble they will be prepared to publish their report, which it is to be hoped will embrace the whole question of units, and fix them on a sound and lasting basis. In order to better illustrate the necessity of fresh measurements, I will direct your attention to the sheet before you marked No. 1.

TABLE I.

Samples of Copper.	Conductivity.		
	1st Test.	2nd Test.	3rd Test.
A	101·4	101·1	101·02
B	44·7	44·87	44·51
C	98·7	99·63	98·64
D	101·3	100·2	101·14
E	18·7	18·63	—

The figures show the specific conductivity of five samples of copper wire, assuming that of pure copper to equal 100, as given by Dr. Mathiessen, viz., 100 inches of annealed pure copper weighing 100 grains, having a resistance of 0·1516 ohms at 60° Fahr. The tests were made by three persons, each one making his tests quite independently, and by means of his own apparatus and method. The results show that, with good apparatus and careful manipulation, very accurate and reliable tests can be made. They also show what great differences there are in the specific conductivity of ordinary commercial copper, and confirm what I have already mentioned, that some commercial copper has a higher specific conductivity than the long recognised standard for pure copper.

Late in the year 1837, Professor Faraday proved by actual experiment that different kinds of matter possessed different inductive capacities, and wrote of "the specific inductive capacity of dielectric bodies." We of to-day use the same language, but our correct knowledge of the subject is very limited, and I believe a careful investigation of the whole matter at the present time would be most valuable. It is customary to consider the specific inductive capacity of air = 1, and to compare the relative capacities of other substances with it. Thus it is often stated

Air = 1·0.
India-rubber = 2·8.
Gutta percha = 4·2.

Now in 1870 I showed that in specially-prepared gutta percha its specific inductive capacity was reduced nearly equal to, if not below that of india-rubber, and from that date the inductive capacity of the gutta percha used for submarine telegraphy has been considerably reduced. When two metals are combined to form an alloy, the resistance of the alloy is in most cases greater than that calculated from the resistance of the component metals and their proportions. I have reason to believe that compounded dielectrics follow the same law, not only in their resistance, but also in their electrostatic inductive capacity. Therefore here is a subject which might also be investigated with advantage. In addition to the electrical resistance and electrostatic capacity of dielectrics, there is the phenomenon termed electrification, absorption, or polarisation. These are synonymous terms, as they all express the same thing. I am sure it would be of great advantage if we could agree to adopt only one of these terms,—polarisation, for instance,—as by so doing it would prevent confusion, and make our meaning less bewildering to our students and other persons. All dielectrics polarise, but in different degrees; in fact, polarisation varies, not only in different materials, but in different qualities of the same material, and also according to its condition. For instance, age and temperature affect polarisation very materially, therefore the formulæ and tables of coefficients given in text-books and elsewhere for polarisation are not only unreliable in certain cases, but misleading when applied to certain dielectrics employed in the construction of condensers, or as the insulating medium in submarine or subterranean telegraphy. The resistance of gutta percha increases gradually by age. Observations extending over twelve years do not enable me to assert that its maximum resistance has been reached within that time.

TABLE II.

Date.	Dielectric Resistance. Megohms.	Inductive Capacity. Microfarads.
February, 1869	214	·385
March, „	261	·386
April, „	302	·389
May, „	328	·391
February, 1870	371	·395
December, 1876	585	·414
November, 1882	632	·419

The figures on the sheet marked No. 2 suspended before you show the increase of resistance of the gutta percha surrounding one mile of a copper conductor, with the dates when tested. Induction also gradually increases in a similar way, as shown. No doubt both these conditions arise from the contraction of the gutta percha round the wire, for, if careful measurements were made, it would be found that the diameter decreases in proportion to the increase in its inductive capacity. Had more frequent tests been taken than are here shown, at temperatures varying from 32° to 100° Fahr., it would still have been found that each series would differ from the other in polarisation, resistance, and induction, and that the formula applied to one series would not be correct for any one of the others. To obtain accurate measurements in experiments of this kind, it is essential that a constant electro-motive force should be employed, but, unfortunately, I know of no means by which such a force can be maintained; for although great and important improvements have been and still are being made in batteries, there has not yet, to my knowledge, been produced one that combines all the essential elements for constancy.

If it were necessary, numerous instances might be given to show how slow the human mind is, and always has been, to comprehend and utilise the rich and wonderful gifts which nature is constantly casting before it. Thales, two thousand four hundred years ago, is said to have discovered what we now term the electric qualities of amber, but it was not until the 17th century that this phenomenon began to attract public attention, and, under the keen observations of the philosophers of that time, to rapidly develop into a science. To the alchemists of the 16th century belongs the honour of having first noticed the change which took place in silver chloride by exposure to light, but photography, which sprang from that very fact, is but of very recent date. It is now nearly ten years (alas! how swift is time) since I first called attention to the wonderful effects of light, and especially solar light, upon the electrical qualities of selenium; but to the present time we have not been able to realise any one of the results which I feel sure will ultimately arise from it; for where there is absorption of light, whether of dark or visible rays, by any body, work of some description must have been performed in that body. Of all the endeavours to utilise this property of selenium, that resulting in the photophone appeared the most promising, but, knowing what I did of the behaviour of selenium, I was not sanguine of its application for the purpose proposed; for as electricity acts by what we know as induction on a dielectric, so does light act on selenium—that is to say, the selenium absorbs it slowly, and as slowly gives it back. My meaning will perhaps be more intelligible by reference to the plate before you marked No. 1. A bar of selenium, in a small room from which all light was excluded, was connected by means of insulated wires to a battery of one hundred cells and an astatic mirror galvanometer in an adjoining room. The vertical figures on the sheet before you represent the ratios of the galvanometer deflections obtained. While the light was excluded, the deflection remained quite steady for ten minutes, as shown at A, but on exposing the bar to the light from an ordinary wax candle placed one mètre from the bar, the deflection immediately increased from 1 to 1·4, and then continued slowly increasing, until at the end of ten minutes it reached 1·45, as shown at B. Immediately the light was extinguished the deflection fell to 1·2, and during the next ten minutes gradually fell to 1·09, as shown at C. The candle was then replaced by a Swan lamp made incandescent by the current from thirty pint Grove cells, and supposed to have an illuminating power of twenty-five candles. The deflection at once increased to 1·51, and during ten minutes gradually further increased to 1·66, as shown at D. On extinguishing the light the deflection immediately fell to 1·27, and during the ten minutes fell to 1·12, as shown at E. Dull solar light was then admitted through an ordinary window, and the deflection at once rose to 2·13, and during the ten minutes further increased, but not so uniformly as when under the influence of the previous source of light, to a maximum of 2·21, as shown at F. The variations in the deflections noticed in this case were due to atmospheric changes producing a varying intensity of light. On again excluding the light, the deflection immediately fell to 1·43, and during the next ten minutes further fell to 1·26, as shown at G. It is evident that it would have taken a much longer time to fall to its normal condition of 1. The fact of having to combine other metals with selenium to obtain a low resistance, which was stated to be essential to the photophone, was also contrary to my experience of its behaviour under the influence of light, but gentlemen eminent in electrical science, having assured me that they had not only seen, but had heard the photophone, made me doubt the correctness of the results of my own experiments. Time has, however, proved that the results then being obtained were not due to the action of light on selenium, but to the then unknown fact that thin discs or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of light—a discovery that has opened a new field for philosophical enquiry; and further experiments have, I believe, shown that the effects produced are not due to light, but to radiant heat. Whether that is really the true cause of so wonderful a phenomenon is not the direct province of the telegraph engineer or electrician to solve, therefore I will not say more on the subject.

Soon after the earth was used as part of the circuit for telegraphic purposes, it was observed that electric currents, very capricious in intensity, direction, and duration, were constantly flowing along the insulated wire of each circuit, the interference of such currents frequently causing great trouble and annoyance. These currents were called "earth currents," but our knowledge concerning them is at the present time very unsatisfactory. To obtain our present knowledge of terrestrial magnetism, observatories had to be established in all the most important parts of the world, to record every movement or manifestation, regular or irregular, of that phenomenon. Might not our knowledge be considerably increased, also, if a similar system could be applied to the investigation of "earth currents" by the aid of the telegraph wires which at the present time extend north, south, east, and west over the world, thus bringing to a focus a number of observations which taken singly are of no value, but when collected and examined by competent men cannot fail to produce beneficial results? I feel sure that, if the importance of the results which would be likely to accrue from such an investigation were clearly put before the several representatives of our telegraphic system, such an investigation might be easily organised. Electricians have done much to obviate the inconvenience caused by earth currents to the practical use of telegraph wires, consequently Telegraph Companies do not interest themselves in the matter to the same extent they no doubt would otherwise have had to do. But we are desirous of obtaining knowledge on the subject, with the higher motive in view of making sure that we correctly link in one chain the aurora borealis, magnetic storms, terrestrial magnetism, and earth currents. If we could with certainty so link them, how easy it might become to link other mysteries of nature in the same chain.

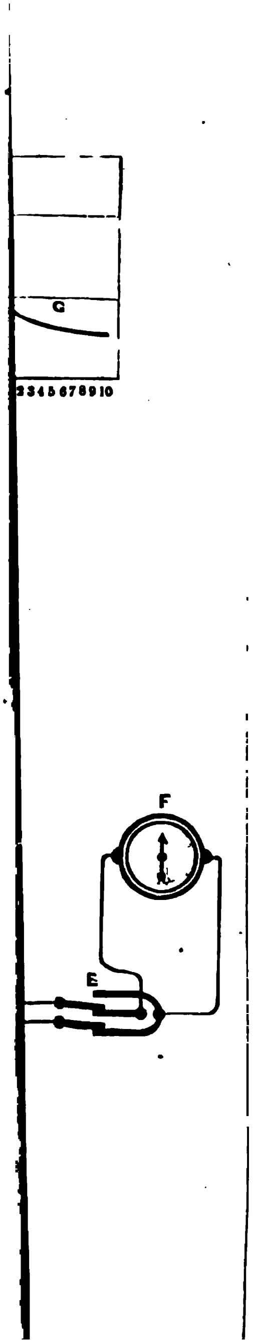
Notwithstanding the great amount of labour and thought that has been bestowed at various times, even by some of the most eminent men, in investigating the properties of the magnet and the various denominations of magnetism, I believe there is still much to be learned, and never was there a time equal to the present when it was so important to possess a perfect knowledge of the subject. It is worthy of note that, although Faraday obtained the first spark by induction from what he termed a common magnet capable of sustaining a weight of one hundred pounds, he on the following day obtained better results from a loadstone capable of sustaining a weight of only thirty pounds. It is generally known that the power of a magnet is always increased by the reaction of another body in which it induces magnetism, and it is the same in a marked degree with the loadstone. If a piece of soft iron be applied to different parts of a small stone which I have in my possession, then, as a rule, where any considerable attraction takes place, but little free magnetism is shown by the iron, but if no attraction exist, then free magnetism is manifested at the end of it. How great an advancement it was to science when the discovery was made that this wonderful power possessed by the loadstone, of the origin of which we are still ignorant, not only attracted iron to it, but that this power could be accumulated and stored in different qualities of iron. If electricity be communicated from one source to another, it leaves the one and gives it to the other; but not so with magnetism, for, however much is taken from one source, no reduction in its power is perceptible. What a marvellous system of storage and what a wonderful power it possesses, when we reflect that the commerce of the world depends almost entirely on a few grains of steel filled with this power that we call magnetism; and yet still more wonderful is the fact that such a magnet can be divided into innumerable pieces, and each piece will be in itself a perfect magnet. What strong contrasts and yet beautiful analogies there are between magnetism and electricity! It was these analogies which led philosophers to seek for proof which they believed would identify the two powers as one: they had from the earliest times of which we have historical record devoted much time to the subject, but it was reserved for Professor Oersted to prove in 1819, by actual experiment, the identity of the two forces. Then the whole thing appeared so clear and simple that the wonder was the discovery had not been made long before. Oersted's discovery consisted in the fact that a conductor of electricity possessed all the properties of a magnet while a current of electricity was passing through it, consequently the real relation between electricity and the magnet is in the current form of electricity. *In fact, it is the stream of electricity itself which possesses the magnetic properties, irrespective of the substance through which it passes.* Of course I need not stop to point out to the members of this Society the important stride this discovery was in the advancement of electrical knowledge. Its effects were soon applied by others in a variety of ways. In 1825, Mr.

Sturgeon showed the great advantage to be gained by using soft iron instead of steel as a core in what were termed electro-magnets, and immediately on that announcement electro-magnets were constructed, the sustaining power of which, in comparison with their weight, far surpassed anything that had been previously obtained.

At the present time the minds of practical electricians are more or less occupied in endeavouring to produce strong magnetic fields for developing high electrical currents for illuminating purposes. In the construction of machines for such purposes, no doubt it is desirable to employ copper wire of the highest conductivity, but it is not so certain as to the quality of the iron best suited to the purpose. It has been asserted on good authority that the worst iron produces the best results, and I venture to think that we have not yet arrived at the best way of placing the wire to obtain the maximum strength of secondary currents from electro-magnets. The results of an experiment made to ascertain the relative strength or capability of producing inductive effects at different parts of a straight electro-magnet is shown on the sheet before you marked No. 2, in which C C, Fig. 1, represents the iron core; P P, the primary coil, connected at pleasure to one Grove cell, B, by means of the key, K; a small secondary coil, S, free to move along the primary coil while in circuit with the galvanometer, G. The relative strength of any particular spot could be thus obtained by moving the coil, S, exactly over the required position, and then noting the deflection obtained on G when the primary circuit was closed by depressing the key, K. In Fig. 2, the figures in black represent the deflections obtained when the soft iron core was used, and the red figures, those obtained when a wooden core was substituted for the iron. I have lately shown, by published experiments with a flat spiral, that it is necessary to place the second coil, which is to be the receiver of inductive effects, in such a position that the lines of force cut it at right angles, in order that the greatest effect may be produced. The effect diminishes as the lines of force cut it more obliquely. An exactly similar result occurs in this experiment, for, as shown in Fig. 3, the small secondary coil is only cut at right angles when it is placed in the centre of the magnet, and as it is moved towards either pole so the lines of force cut it more and more obliquely. From that it would appear that the results obtained are not purely dependent upon the strength of that portion of the magnet over which the secondary coil is placed, but principally upon the angle at which the lines of force cut the coil when so placed. It does not therefore follow that the centre of a magnet is its strongest part, as the result of the experiment at first sight appears to represent it to be.

In my published experiments, just referred to, on what I termed specific inductive resistance, the instruments employed in one of the experiments were arranged as shown on the sheet before you marked No. 3. A and B represent two flat spirals of silk-covered copper wire fixed a suitable distance apart, A being in circuit with the battery C and reverser D, whilst spiral B is in circuit with a similar reverser C, and an astatic mirror galvanometer F. As the two reversers were made to work in unison, it will be readily seen that the currents induced in spiral B by A would always be in the same direction in the galvanometer circuit, and accordingly a steady deflection was obtained proportional to the inductive effect received by B. A plate of the metal under examination was then placed midway between the spirals at G, and the fall of the galvanometer deflection noted, this fall being due to the interception by the metal of the inductive lines of force. It was found that the results varied in a remarkable degree according to the number of reversals per minute, each metal having its own characteristic. These experiments have been continued, and the results are to me so very interesting that I have ventured to place some of them before you this evening. On the sheet before you, marked No. 4, the vertical column of figures represents percentage of inductive radiant energy intercepted, and the horizontal row of figures the number of reversals per minute. The various lines represent the results obtained with each metal examined, and it will be seen that copper, though unable to intercept to any extent the inductive lines of force when a slowly intermittent current is flowing in spiral A, sheet 3, is able to arrest ninety-eight per cent. when the reversals are sufficiently rapid. It will be readily seen that the iron behaves in a different manner to the other metals, and that in a greater or less degree every metal has its own characteristic. The metals not only intercept the rays of inductive energy by means of absorption, but bend them from their original path, and the amount of this refraction varies, as might be expected, with each metal, and also with the rapidity of the reversals in the inducing spiral. The refraction was measured by means of two flat spirals, one in connection with a battery and reverser, as spiral A, sheet 3, and the other, or receiving spiral, in circuit with the reverser and galvanometer, as shown at B in the same sheet, but in this case the receiving spiral was placed in a position parallel to the lines of force, and consequently was unaffected by them, until, by placing the metal under examination in the field, refraction took place, and the lines of force, being then no longer parallel to the receiving spiral, induced a current therein proportional to the refraction produced. Copper has the greatest refractive power, and iron, except for low speeds of the reverser, the least; and also in the case of iron the refraction remains almost unchanged for any speed, so that here again we have a marked contrast between iron and the other metals. I merely mention these facts in passing, but hope at some future date to bring them more prominently before you.

Although many eminent men of science had long noted the analogy between lightning and electricity, it was reserved for Franklin to demonstrate its identity by a bold experiment in which he proved that lightning and electricity were one, and consequently obedient to the same laws. Franklin at once conceived the idea of protecting buildings and other high structures from the effects of lightning, by simply erecting on their highest parts a pointed conductor the other end of which should be well buried in moist earth, and thus the electricity of the atmosphere would be silently and harmlessly dispersed. Does it not appear strange that so sound a principle, so easy in application and so universally beneficial in practice, should, after all that has been written and said on the subject for the last one hundred and thirty years, be still so imperfectly understood by the general public as to . the Council of the Meteorological Society in forming a com: "to consider the possibility of formulating the existing ledge on the subject of the protection of property from d: by electricity, and the advisability of preparing and issu general code of rules for the erection of lightning conductors committee was formed of gentlemen well qualified for such and the result of their labours was published in the early p last year. The report is not so clear on several points as co desired, and it is to be regretted that the committee thou advisable to append to their report so much useless matter, v had they sifted and only formulated that which was importa the subject, would have proved more intelligible to the ge public, for whose instruction I presume it was intended. the simple requirements for an efficient conductor are not ciently understood by all persons who style themselves elec engineers, impressed itself strongly upon my mind through cumstance which lately occurred within my own experienc was informed that an "electrical engineer," who had been hauling old, or erecting new conductors to shafts in the imme neighbourhood, had, after pointing out that the conductor particular shaft had not the points sufficiently high above brick work, and that the rod not being insulated from the chir was of but little if any use, made a reasonable offer to repla by one erected on the most scientific principles. On discus the question with him, I found that the only reason he could in justification of the correctness of his criticism was that was the custom to so erect them in his part of the coun As the conductor in question had been doing duty for th years, I thought perhaps it would be at least prudent to ha new one on "pure scientific principles," but I declined to hav insulated, and arranged for the bottom end to be attached sheet of copper immersed in a canal adjoining the premises. he considered quite unnecessary, "for a wooden tank placed at base of the stack filled with water, and the end of the conduc placed in it, would be more safe, and certainly less costly." work was, however, done as I desired, and as far as I know is do


G
2 3 4 5 6 7 8 9 10
F
E

its duty; but I noticed that soon after its erection the top assumed a horizontal position from the force of the first strong wind. Fortunately for us, electrical storms are not so frequent or so severe in this as in other countries, and that no doubt is the reason for the great indifference to, and ignorance of the subject. But still lightning has done harm in town and country, therefore our object should be to provide means by which the current can be carried off without any injury. The fact that there is no part of a lightning conductor when charged that will not give lateral sparks to other conducting bodies in its immediate vicinity should not be lost sight of, and, that being so, how essential it is, especially at the present time when iron performs such an important part in buildings in the form of girders and roofs, that the whole should be connected to one efficient conductor. Should electric lighting make that progress which I believe it will, there can be no doubt that responsible and qualified persons will have to be appointed to control the arrangement of at least out-door conductors, and then perhaps it may be found desirable to place the erection and supervision of lightning conductors in the charge of the same persons.

Early in 1825, Faraday began to unfold a most important secret of nature, which marked a distinct and important epoch in the history of electrical science. By actual experiments he demonstrated the wonderful effects of the various denominations of induction, more especially of that which he designated "magnetic-electric-induction." In a letter, now in my possession, written by him, dated November 25th, 1833, he states: "I first obtained the spark with a common magnet (*i.e.*, Dr. Knight's) at Woolwich, February 8th, 1832, and the next day (*i.e.*, on the 9th) from Mr. Daniell's loadstone." Although in the effects shown by Faraday the intensity was feeble and quantity small, he then asserted that, as the effects could be produced by electro-magnets, there could be no limit to exalting the force he had already obtained, and that he was sure of its full development hereafter. In fact, Faraday then produced the germ which is now fast developing into a great branch of knowledge and commercial enterprise, especially in what is termed electric lighting.

Men, finding that this power could be produced, and observing that it could be transferred, set to work to manufacture machines which should evolve strong currents; while philosophers at home and in every civilised country repeated the experiments, and vied with each other in adding new facts to those already known. M. Pixii exhibited in Paris, in September, 1832, a magnetic machine—that is, an instrument by which a continuous and rapid succession of sparks could be obtained. In the following year Mr. Saxton exhibited at the Adelaide Gallery, London, a machine which differed from that of Pixii's in that in his machine the coils revolved round a stationary magnet, while in M. Pixii's the magnet revolved and the coils were stationary. Other machines soon followed, on the same principle of course, but varying in the situation of their several parts, the most notable among which were those of Mr. Clarke, Dr. Ritchie, and Professor Henry. In those early machines the object appears to have been to obtain the best results for exhibiting the most intense spark and terrifying shock, more for curiosity as a scientific toy than for any practical purpose. Professor Holmes, in 1853, constructed the first magneto machine powerful enough for lighthouse illumination. It consisted of thirty-six compound magnets, which rotated six hundred times per minute in front of one hundred and twenty helices, so connected as to produce direct currents. In 1857, an experimental trial of this machine was made by the Trinity House at Blackwall, under the direction and to the great delight of Faraday, who reported, that in his opinion Professor Holmes had practically established the fitness and sufficiency of the magneto-electric light for lighthouse purposes, the light produced being powerful beyond any other that he had yet seen so applied. Three years later, referring to this class of machine, Faraday said: "For the last six months the South Foreland has been shining by means of this electric light. It has shone into France, and has been seen and taken notice of by the authorities there. Never once during six months has it failed in doing its duty." Great improvements followed in machines for developing the current, but utilising it for illuminating purposes made little progress, owing to the difficulty in producing a practical form of lamp, until 1878, when, by the introduction of the Jablochkoff lamp or candle, there shone in Paris a blaze of electric light that astonished all who beheld it. It was thought that the long-difficult problem was at length solved, and men returned to the subject with renewed energy; and the satisfactory results of their labours were shown in the electrical exhibition held in Paris in 1881, and nearer home in the following year, at the Crystal Palace. But great as has been the advancement in the improvement of both machines and lamps, they are still far from what could be desired. The best arrangement will be that in which the minimum quantity of coal energy is converted into the maximum mechanical energy, and consequently the maximum electric energy. By increasing the velocity of the rotation of the coil or coils, the electro-motive force could be increased to an extent yet unknown, owing to the mechanical difficulties which arise if a given velocity be exceeded.

In the preparation of carbon for the lamps, whether in the form of filaments for the incandescent light or in the form of rods for the arc light, it is important that it should be as pure as possible. For incandescent lights, filaments of vegetable fibre, either natural, as bamboo-fibre, or in a manufactured state, as parchment-paper, thread, or paper, are carbonised free from oxygen, by which means the oxygen and hydrogen of the fibre are driven off, and the carbon remains, which is infusible and unalterable at high temperatures. But frequently the inorganic constituents of the fibre, consisting of silica, iron, lime, and other bases, are not driven off during the preparation, consequently the glass becomes dim, the carbon disintegrates, and after use for a short time breaks. In the arc light the carbon rods are consumed, and the silicate and other constituents of the ash fuse and accumulate until they drop, causing, in combination with defective regulation of the lamp, what is known as the flickering or jumping of the light. The method which will best obviate these objectionable sources of annoyance will certainly be the approved system. The numerous appliances for regulating the distance of the carbons for the arc light display great ingenuity, but still there is much room for improvement in that direction, and we should not forget that the means of developing the electric current are considerably in advance of those for utilising it in the present forms of lamps.

The conductivity of conductors decreases in well-known ratios with an increase of temperature, consequently it is essential that conductors, especially those used for electric lighting, should be of sufficient capacity, so as not to be affected by the current. If the current passing along a conductor increases the temperature of the same, it is plain that the resistance of such a conductor is too great, and therefore its temperature and resistance will continue to increase until very prejudicial effects are reached. Perhaps the results of actual experiment will make this more clear. The experiments were made with a dynamo, the resistance of the armature of which was 0·6 of an ohm, and an arc lamp, the resistance of which was 0·38 of an ohm. In each case the circuit was metallic, and the total length of the conductor fifty yards, insulated to a thickness of ·065 of an inch. At one part of the circuit the conductor leading to the lamp and that leading from it were placed so as to touch each other, the consequence of which was as follows:—When the resistance of the conductor was ·28 of an ohm and the strength of the current 27 ampères, the dielectric became soft and allowed the conductor to form contact in twenty-five minutes. With a conductor of 0·21 ohm resistance and a current of 30 ampères, the same results followed after forty minutes; but with a conductor of ·07 ohm resistance and a current of 33 ampères, no heating effects were perceptible during the two hours the lamp was kept burning. In another experiment a tube containing mercury formed part of the circuit, from the centre of which projected a much smaller tube, so that if the mercury expanded it would ascend into this smaller tube and form contact with a wire suspended from the top, and thus complete a local circuit which was so arranged as to either ring a bell or shunt a portion of the main current, if the temperature exceeded a certain limit. The results obtained by this arrangement were as follows:—

Ampères.	Distance between wire and mercury in small tube at starting.	Time before bell rang.
33·6	·125 of an inch.	15′
32	·250 „	45′
32	·375 „	140′
32	·5 „	At the end of six hours the mercury had not expanded sufficiently to reach the wire, consequently the bell did not ring.

If it be a fact, as I believe it to be, that metal conductors charged with electricity fuse at a much lower temperature than that required to melt them when not so charged, how important it is that all the knowledge possible should be gained on the subject, especially by all interested in the distribution of powerful currents. That the electric light is to be the light of the not distant future, no person acquainted with the subject can for a moment doubt. Of course it will require time for its development, but I must admit that the progress it has made within the last four years has astonished me. A great demand was suddenly made on a totally inadequate supply of practical men, and it was appalling to see how inexperience rushed in where experience feared to tread. The only wonder is that greater mischief has not been done to retard its progress. The desired experience, however, is being hourly and daily obtained, which must eventually lead to perfect success, for, in the language of Faraday, "such is the beauty of the manifestations that come before us when we deal with the great laws of nature and observe phenomena which never fail, because the Creator governs by fixed laws. He has permitted us to examine them and admire their effects, and make provisions for their application to purposes useful to man." In measuring the illuminating power of light, it is customary to adopt the simple method suggested by Count Rumford, which consists in arranging the two lights to be compared, so that each casts a shadow of some object on a white surface, and the eye has to judge as to the relative density of these shadows. The brightest light, which casts the deepest shadow, has then to be removed farther from, or the weaker one brought nearer to the screen, until the two shadows are pronounced equal in density, and the distance of the two lights from the screen being measured, the relative intensity of the lights is supposed to be as the square of the distances. But I found, when comparing a standard candle with an incandescent electric light, that the results differed in proportion to the distance the standard light was placed from the screen, and also that the variable state of the atmosphere influenced the results. Again, the eye is not more to be depended upon for an estimate of light than the hand is for the weight of an object, particularly when any difference of colour may tend to perplex the judgment. I, therefore, am not surprised at the diversity of opinions often expressed as to the relative values of different lights. An accurate photometer, or measurer of light, formed on other principles than that of the judgment of the eye would be a great desideratum at the present time.

Early in the days of voltaic electricity, many minds were occupied with endeavouring to discover the best mode of developing the same. Professor Ritter constructed a pile of discs of copper and moistened card, placed alternately the one on the other. It was found, however, that such an arrangement had no power of developing electricity by its own action, but was capable of receiving a charge from a voltaic pile, and parting with it very slowly. This discovery attracted the attention of other experimenters, but no great results were produced until Planté used plates of lead immersed in a solution of sulphuric acid and water. Such combinations were called "secondary batteries," but no lasting practical use was ever found for such batteries. When electric lighting began to advance with giant-like strides, it was predicted that the inability to store the current would be a fatal barrier to its success. Electricians were fully alive to the importance of this objection, and nothing was more natural than that they should have directed their attention to "secondary batteries," as most likely to afford a means of storage. In the spring of 1881 it was announced that by such means the difficulty had been overcome, and that "a box of electric energy, equivalent to nearly a million foot pounds," had been transmitted from Paris to Scotland in perfect safety. This announcement was shortly afterwards confirmed by one who always commands our attention whenever he either writes or speaks on such subjects. This sudden and astonishing, but somewhat ambiguous statement naturally attracted a great deal of attention, which soon became impatient curiosity when it was found, as is usually the case in like matters, that a certain amount of mystery and caution attended the whole thing. When it was at length known that the results stated were obtained by simply covering the lead plates of a Planté secondary battery with oxide of lead, similar to the plan tried by M. De la Rive when experimenting on the same subject in 1843, no doubt many experiments were made in that direction. The results of my first experiments were published in the September of 1881. The question I endeavoured to solve was the durability and cost of such batteries, and I feel bound to state that the results obtained were not in favour of that class of battery. On several occasions statements have appeared which were not in accordance with my experience, and which, emanating from the source they did, made me doubtful of the correctness of the results of my own experiments. But two years have now passed, and I know of no instance in which these batteries have gone beyond the experimental stage, or accomplished anything that could not have been done by an ordinary battery; therefore we can only infer that they have not proved so successful as we were led to believe they would, and at the present time we must admit that the difficulty of storage has yet to be overcome.

All interested in the successful progress of submarine telegraphy must be pleased with the satisfactory advancement that branch of our science has made and is still making. Twenty years ago opinions differed as to the practicability of deep-sea telegraphy as much as they do at the present time concerning the ultimate success of electric lighting. The reasons asserted, even by persons whose words carried a certain amount of authority, against the possibility of ever successfully establishing long lengths of submarine cables, were too absurd for sober record. "Unfathomable depths, where, if a cable found a resting place, the enormous pressure to which it would be subjected would destroy its component parts." "A cable would never descend beyond a certain depth." "There were in the ocean certain huge monsters which would devour the cable as rapidly as it was laid." Opinions as to the electrical difficulties in working such cables were as equally conflicting. At the present time, however, with but few exceptions, every civilised part of the world is telegraphically connected, the imaginary difficulties vanishing as submarine telegraphy advanced; for not only have cables been successfully submerged in great depths, but, after many years, have been lifted for repairs with as little difficulty as there was in laying them. It has also been proved that high speeds can be obtained with conductors of high resistance, as compared with those first considered necessary. I believe electric lighting, which at the present time is engrossing so much attention, will progress in the same satisfactory way in the hands of competent and sincere workers.

The telegraph system of England under Government management would, I believe, compare favourably with that under the control of any other government, but, considering the disorganisation that occurs whenever we are visited with severe storms or strong electrical atmospheric disturbances, there is yet much room for improvement in that direction. With a view to prevent such obstructions, both in France and Germany, the aërial lines are being superseded by subterranean wires, with, as I am informed, satisfactory results.

The telephone, in conjunction with that equally marvellous instrument the microphone, is making progress as a telegraph instrument, but up to the present time only for comparatively short distances, owing to its very sensitive nature. In experimenting with other telegraph instruments, it is, as a rule, sufficient to get at their working capacity by means of resistance coils, but not so with the telephone. I have heard distinctly with a telephone through a metallic circuit of two megohms, which was equal in resistance to 330 Atlantic cables, or 664,000 miles, but when the circuit was changed to a cable 68 knots in length, having a resistance of 675 ohms on one end of the telephone, and a resistance equal to that of the cable on the other, the sounds received were very indistinct. On the 8th of January, 1880, Charing Cross and Cannon Street stations were telephonically connected by two special wires, suspended so as to be as far as possible from the influence of the ordinary telegraph wires. Notwithstanding the combination of noises generally heard in either of these stations, conversation could be freely carried on, and I at Charing Cross heard distinctly the ticking of a watch which was placed on the instrument at Cannon Street. Greater wonders than this have been achieved with the same kind of instrument since that date, and I merely instance it as a fact in the early days of the microphone. As at present constructed, the telephone is far too sensitive to compete successfully with other types of telegraph instruments, but it would be interesting to know at what distance they really would work on a circuit, either earth or metallic, placed far from any external influence. Owing to the inductive effects of submarine cables, the distance would be comparatively limited.

The calls on the skill and inventive faculties of telegraph engineers and electricians have increased so rapidly, and are so numerous and multifarious, that I have found it impossible in the time allotted me this evening to do more than just touch upon a few of the most important.

In conclusion, I would remark, especially to the younger members of this Society, that it has always been my endeavour to follow the advice of an author whose name I have forgotten, but who wrote: "I find a frank acknowledgment of one's ignorance is not only the easiest way to get rid of a difficulty, but the likeliest way to obtain information, and therefore I practise it. I think it an honest policy. Those who affect to be thought to know everything often remain long ignorant of many things that others could and would instruct them in if they appeared less conceited." The tendency of the present day appears to be to sacrifice much to the love of gain. We have an honourable calling, then let our endeavour and ambition be to guard it as such, and, in so doing, "learn to labour and to wait" for that reward which is sure to crown the labours of the honest worker.

Professor W. Grylls Adams, F.R.S.: Gentlemen,—I have in one sense a very easy duty to perform, because I see that you are entirely with me in what I should wish to say, although the terms in which I do it may not be so well chosen as I could wish.

Our President has put before us various very interesting and very important points which are worthy of the careful consideration of practical men, and indeed of all those who are interested in electrical science: such, for instance, as the conductivity of copper, the action of light on selenium, the inductive action between coils of wire placed far apart from one another, and the effects of introducing screens of different metals between those coils of wire. He has also treated of many other important points connected with the practical application of electricity, and we look to him, of course, as one of our highest authorities on all matters relating to Atlantic cables and their history. Among other points touched upon, there was one in which I was very deeply interested, for I rejoice to say that in our Museum at King's College we have the loadstone, spoken of by our President, which has been made celebrated by Faraday's experiments with it—the loadstone which was lent to Faraday by Professor Daniell, and which was the first natural magnet from which Faraday obtained his induction spark. It always gives me great pleasure to show it to those who are interested in the subject. The loadstone referred to weighs 28 lbs., and is just capable of supporting its own weight. I am sure we have all been very much interested to-night in the address of our President, and in the various topics which he has introduced into it, and I ask you to join with me in thanking him, and to give your cordial support to the proposal which I have to make: "That the thanks of the Society are due to the President for the very interesting address which has been delivered by him, and that he be requested to permit its publication in the Society's Journal."

Mr. C. E. **Spagnoletti** seconded the proposition, and cordially endorsed Professor Adams's remarks. Out of the many interesting points touched upon in the President's address, he (Mr. Spagnoletti) enquired whether some practical advantages might not be derived from further enquiry into the peculiar properties of selenium for the purposes of measuring lights of great power—a subject which had hitherto been left in a very unsatisfactory state, in spite of the trouble and attention given to it. Some scale could probably be laid down on the principle of the one which accompanied the President's paper, to show that the varying resistance or conductivity of selenium was in accordance with the candle-power of large or small lights presented to it, and so a reliable photometric instrument may be obtained. He thought that the older members of this Society, as well as the younger ones, might advantageously adopt the President's remarks in regard to the admission of want of knowledge where information was required or desired by them.

The vote was heartily accorded.

The **President**: Gentlemen,—I am extremely obliged to you for the kind way in which you have received my address. I am afraid I have not delivered it as clearly as I should have wished to have done, but if I have said one single sentence which will in any way conduce to the advancement of the science in which we are all so deeply interested, I shall be more than amply repaid for all the time and thought it has cost me in compiling.

A ballot then took place, at which the following new members were elected:—

As Foreign Members:

Tadasuke Ishie.
R. Shida.
His Excellency General W. de Raasloff.

As Members:

Thomas Buckney, F.R.A.S.
Charles Capito.
Alfred Clayton.
Charles Friend Cooper.
J. Angelo Fahie.
R. D. Smillie.
Ivor C. Thomas.

As Associates:

G. H. Benson.
John Brailsford Bright, M.A., Oxon.
Lieut. M. A. Cameron, R.E.
Edward Catley.
Harold Collet.
Thomas Marshall Collet.
Henry Crookes.
Richard Lewis Cousens.
W. F. Dennis.
Bernard Mervyn Drake.
Captain Arthur Briscoe Hawes.
Lieut. Henry B. Jackson, R.N.
Lieut.-Colonel Keyser, C.B.
Leonard Milne, B.A.
Percy Hartley Naftel.
Frederick Baumgart Nicholson.
Major S. Flood Page.
Alexander Pentland.
Ernest Cecil Rimington.
Lieut. Buchanan Scott, R.E.
Lieut. Andrew Stuart, R.E.
Señor Rafael Sunyé.
H. J. Vose.
Charles Clement Vyle.
Benjamin Warwick.
John Richard Williamson.
Frederic Wyles.
Fung Yee.

As Students:

Frederick Benjamin Oliphant Hawes.
Eugene Joseph Moynihan.

The meeting then adjourned until Thursday, February 8th, 1883.

The One Hundred and Eighteenth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 8th, 1883—Mr. WILLOUGHBY SMITH, President, in the Chair.

The minutes of the previous meeting were read and confirmed. The names of transfers and of new candidates were announced and suspended. Transferred from the class of Associates to that of Members:—

G. P. Simpson, B.Sc.

From the class of Students to that of Associates:—

L. D. Barclay.

The following recent donations to the Society's Library were announced:—

Director, Central Italian Telegraphs; Iron and Steel Institute, Siemens and Halske, Latimer Clark, Gannini Alamiro, Kew Observatory.

The SECRETARY then read the following paper:—

ON THE MAGNETIC STORM OF NOVEMBER 17TH, 1882.

By J. GRAVES, Member.

On arrival at the office of the Anglo-American Telegraph Company, Valentia, on Friday morning, the 17th November, I found all lines east and west stopped by earth currents of extraordinary strength; the siphons of the Thomson recorders on all cables changing from side to side off the paper slip, and assuming frequently a horizontal position. Simultaneous observations on all four cables showed synchronous changes; but while the three western cables recorded a positive current, the Emden cable eastward showed a negative current, and *vice versa.*

Finding that working was impossible, I took the 1873 cable and made observations upon the quadrant electrometer, the constant of which at the time was 25 Fuller's bichromate cells, equal to 300 scale divisions; a positive charge in the cable giving a right-hand deflection from zero, and a negative charge a left-hand deflection.

Upon first connecting the cable to the electrometer, the reading was positive, and varied subsequently as indicated in the following figures. The readings were taken as rapidly as they could be read and written down. The time of vibration of the needle being short compared with the intervals between the readings plotted, the curve may be taken as unaffected by those vibrations:—

Time.	Deflection.	Direction.
a.m.		
10.55	170	Right.
10.56	260	Electrometer violently agitated—not much dependence to be placed upon these preliminary readings.
10.57	370	
10.58	400	
	500	
10.59	600	
	700	
11. 0	800	
11. 2	900	
11. 3	1,000	Limit of mirror. Violent oscillations settling down at Right.
11. 5	40	
	70	
	35	
11. 6	40	
	10	Left.
	35	
	60	
	100	
	150	
11. 7	170	
	200	
	160	
	210	
	235	
11. 8	245	
	260	
	300	
11. 9	...	Zero checked.
	50	Left.
	100	
	150	
11.10	200	
a.m.		
11.10	100	
	50	
11.11	150	
	170	
11.12	150	
	160	
	100	
11.13	90	
	120	
	110	
	150	
11.14	80	
	200	
	240	
11.15	280	
	250	
11.16	220	
	250	
	270	
11.17	...	Zero checked.
	250	Left.
	300	
	260	
11.18	210	
	190	Suddenly crossed over.
11.19	50	Right.
	100	
	180	
	230	
11.20	270	
	310	
	350	

Time.	Deflection.	Direction.	Time.	Deflection.	Direction.
a.m.			a.m.		
11.20	340		11.29	70	
	360		11.30	100	
11.21	400	Extended scale.		110	Crossed suddenly.
	500	Right.	11.30½	100	Left.
	600			130	
	500			90	
11.22	400			150	
	350		11.31	160	
11.23	400			200	
	500			215	Trembling.
	520			205	
	500			210	
11.24	400			215	
	350		11.32	220	
11.25	370			210	
	390			215	
	380			250	
	390		11.33	240	
	430			225	
11.26	450			230	
	430		11.34	215	
	470			220	
	430			50	Dropped suddenly.
	350		11.35	70	
11.27	300			100	
	150			30	
	170			50	
	160			10	
	150			25	
	170		11.36	0	
	210			50	Right.
	250			5	
11.28	300	Trembling.		30	
	310			120	
	260			0	
	200			50	Left.
11.29	150			0	
	130			60	Right.
	165		11·37	40	
	100			10	
	50			40	

Time.	Deflection.	Direction.	Time.	Deflection.	Direction.
a.m.			a.m.		
11·37	0		11·48	270	
	40	Left.	11.49	260	
	0			250	
	60			260	
	0			280	
	40	Right.		260	
	0			250	
	60	Left.		280	
	30		11.50	230	
	50			200	Fell suddenly.
	15			60	
11.38	55		11.51	0	
	0			20	Right.
	20	Right.		30	
	0			20	
	40	Left.		0	
	80			50	Left.
	60		11.52	70	
11.39	100			125	
	70			80	Violent trembling.
	140		11.53	55	
	110			150	
	140		11.54	170	
	170			190	
	165			150	
11.40	220			170	
	170			120	
	180		11.55	80	
	130			50	
	150			40	
11.41	80			100	
	125		11.56	130	
	110			90	
	125		11.57	100	
	165		11.58	90	
11.42	150			140	
	200			200	
11.43	235			240	
11.44	260		11.59	270	
11.45	270	Steady 3 mins.		290	
11.48	310		Noon	270	

At noon joined up cable for working, but found it to work, the siphon on this, as also on other cables quently in a horizontal position.

At 12.42 p.m. the currents ceased on the land gradually decreased on all cables.

At 12.45 p.m. all cables at work.

From 3.24 to 3.47, 4 to 4.15, 4.40 to 5.20, 5.26 to and at frequent later intervals during the evening, currents continued to interfere with working.

On November 18th these disturbances were renewe the morning, and two of the cables were worked in me as had been done on the previous evening.

Land lines east of Valentia and west of Newfound more or less stopped all day on the 17th, and a grea work remained on hand in New York for the eastern he

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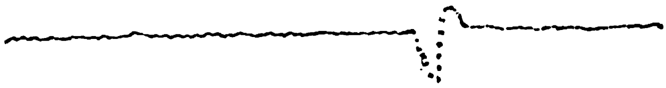
SIMULTANEOUS EFFECTS OF LIGHTNIN ON DIFFERENT CABLES.

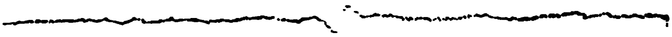
By J. GRAVES, Member.

On the 8th November, 1882, lightning was pre Valentia, and the following slips show the record different cables :—

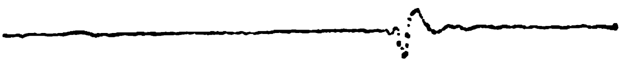
On 1873 cable—



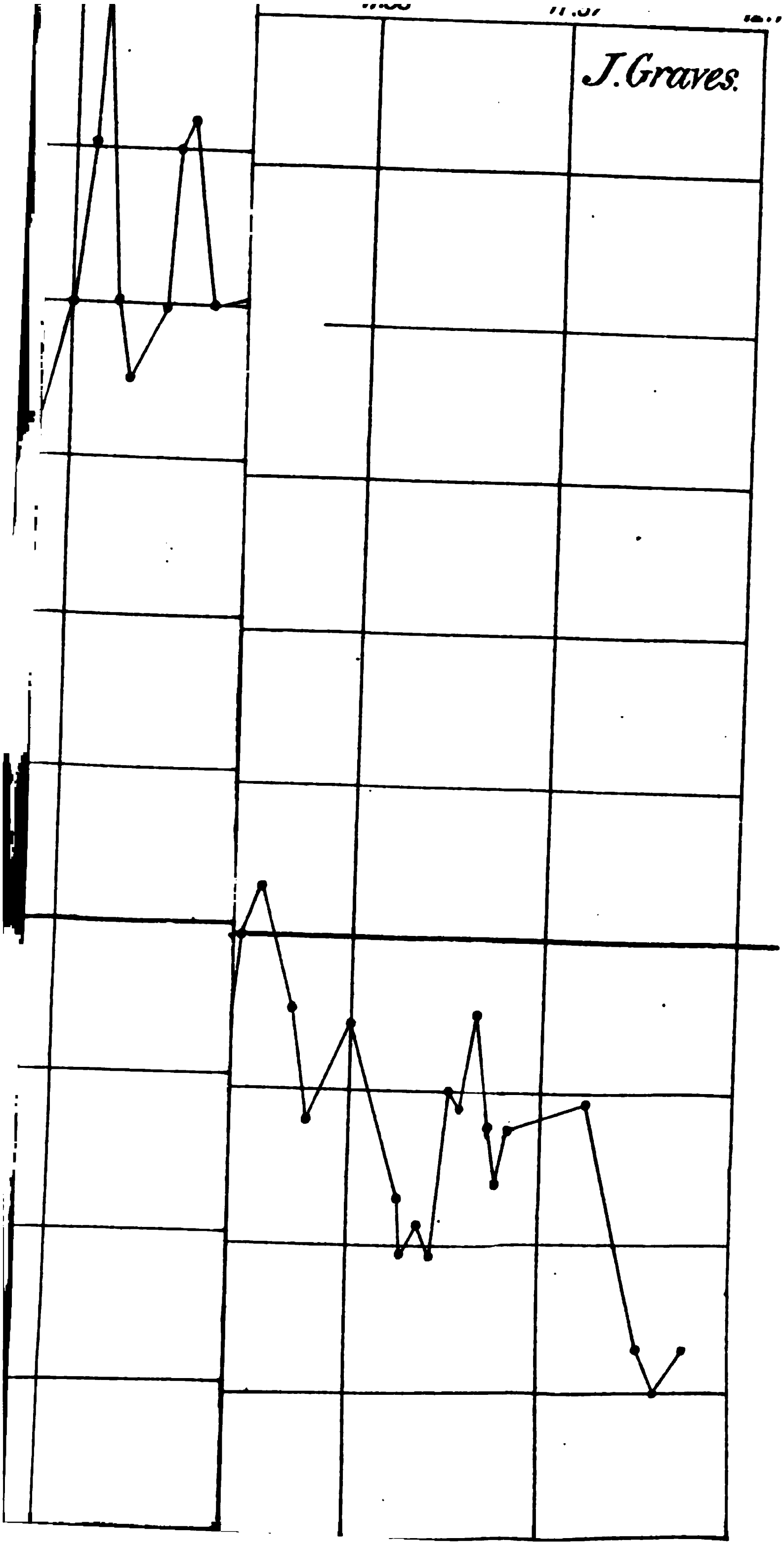
On 1874 cable—



On 1880 cable—

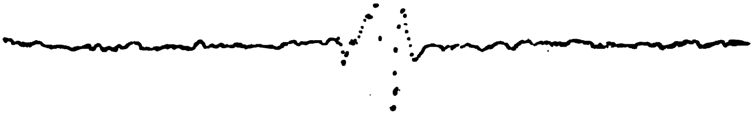


There was a heavy thunderstorm with very frequent a flashes of lightning here on 12th February, 1883, all cabl

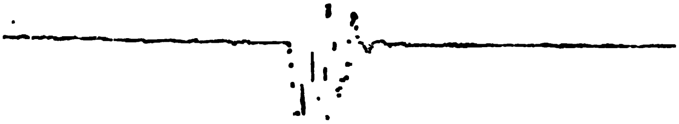

J. Graves.

affected by "kicks" due to inductive effects from the lightning discharges. At 1.55 p.m. the following simultaneous effects were observed:—

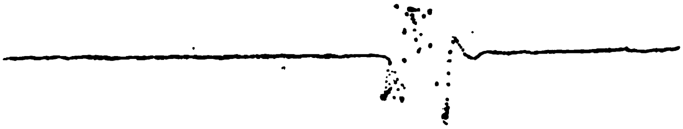
On 1873 cable—



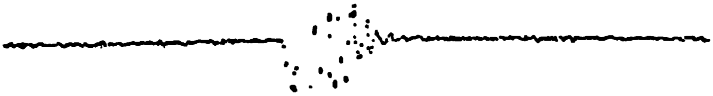
On 1874 cable—



On 1880 cable—

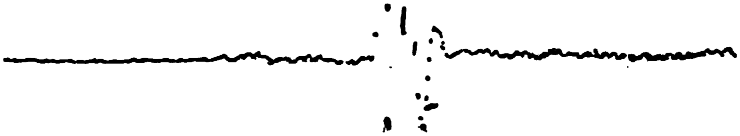


On Emden cable—

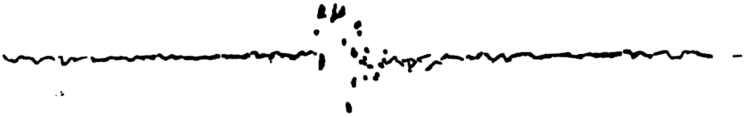


At 2 p.m. another vivid flash occurred, and gave the following records on all cables:—

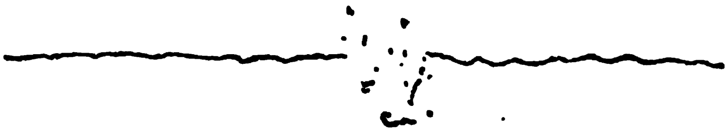
On 1873 cable—



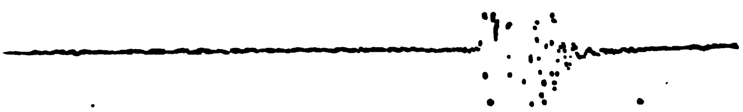
On 1874 cable—



On 1880 cable—

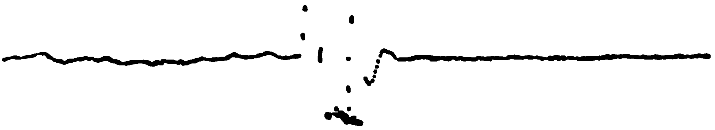


On Emden cable—



It so happened that we were not receiving at these times.

At about 2.4 p.m. a third severe flash occurred, giving the following record on the 1880 cable:—



and similar indications to the preceding ones on the 1873 and Emden cables; but on the 1874 it gave me something else to do than to collect records, for the 1874 suspended coil of the recorder was fused. Valentia was sending in duplex at the time, and the sudden loss of one circuit, together with the effect of the lightning, snapped the platinum wire carrying the siphon, and thus thoroughly wrecked the instrument, necessitating the insertion of a new suspended coil, and the renewal of all fibres, etc.

I remember being asked (I believe by Mr. W. H. Preece) whether I ever noticed lightning to affect our instruments, and therefore these details may perhaps interest some one or more of our members.

The ocean cables are joined to the station by 5¾ statute miles of underground wires, in trenches of 2 feet to 3 feet in depth.

NOTE BY W. H. PREECE.

These observations are extremely interesting. They show the frequent reversals induced by a discharge of atmospheric electricity which produce letters on the Morse apparatus. They do not quite answer the query I once raised, which was this : Do thunderstorms out at sea, beyond the reach of the ear or eye, produce "kicks" on the cables?

The SECRETARY then read the following two papers:—

MAGNETIC STORM IN INDIA.

By E. O. WALKER, Member.

A magnetic storm was observed in India on the 12th September last. Special readings were taken of the current in the lines from Belgaum to Vingorla (70 miles), and from Belgaum to Bellury (200 miles). These lines run approximately east and west.

In the chart the horizontal lines express the strength of current in milliwebers, each space corresponding to 0·2 milliweber. The vertical lines divide the chart into minutes, from 7 to 11.9 a.m. The broken curve is the daily normal earth current; the thick curve is the storm current on the Belgaum-Vingorla line; and the thin curve the same for the Belgaum-Bellury line. Curves above the centre horizontal line show the current flowing from west to east; those below, from east to west.

The storm began about 7 hours, and by 15 hours had disappeared. Observations were suspended after 11 a.m., as the wires were required for traffic. At this time the storm had moderated. It was coincident with a cyclonic disturbance in Guzerat, and a very heavy rainfall north of Bombay, and the approach of the end of the south-west monsoon. A line running east and west from Kolhapur to Ratnagiri (77 miles) was affected in the same way and to the same extent as that from Belgaum to Vingorla. The wires from Kolhapur to Belgaum (69 miles), which lie north and south, were almost free from interference, the observed current being from 0 to 0·689 milliweber from north to south. As a rule, the line from Belgaum to Bellury was affected more quickly and to a greater extent than that to Vingorla, though the currents in both lines preserved a fair uniformity of direction.

The wires of the lines above mentioned may be taken as 5½ B.W.G., and the relays at both ends of each line were in circuit when the observations were made.

EARTH CURRENTS IN INDIA.

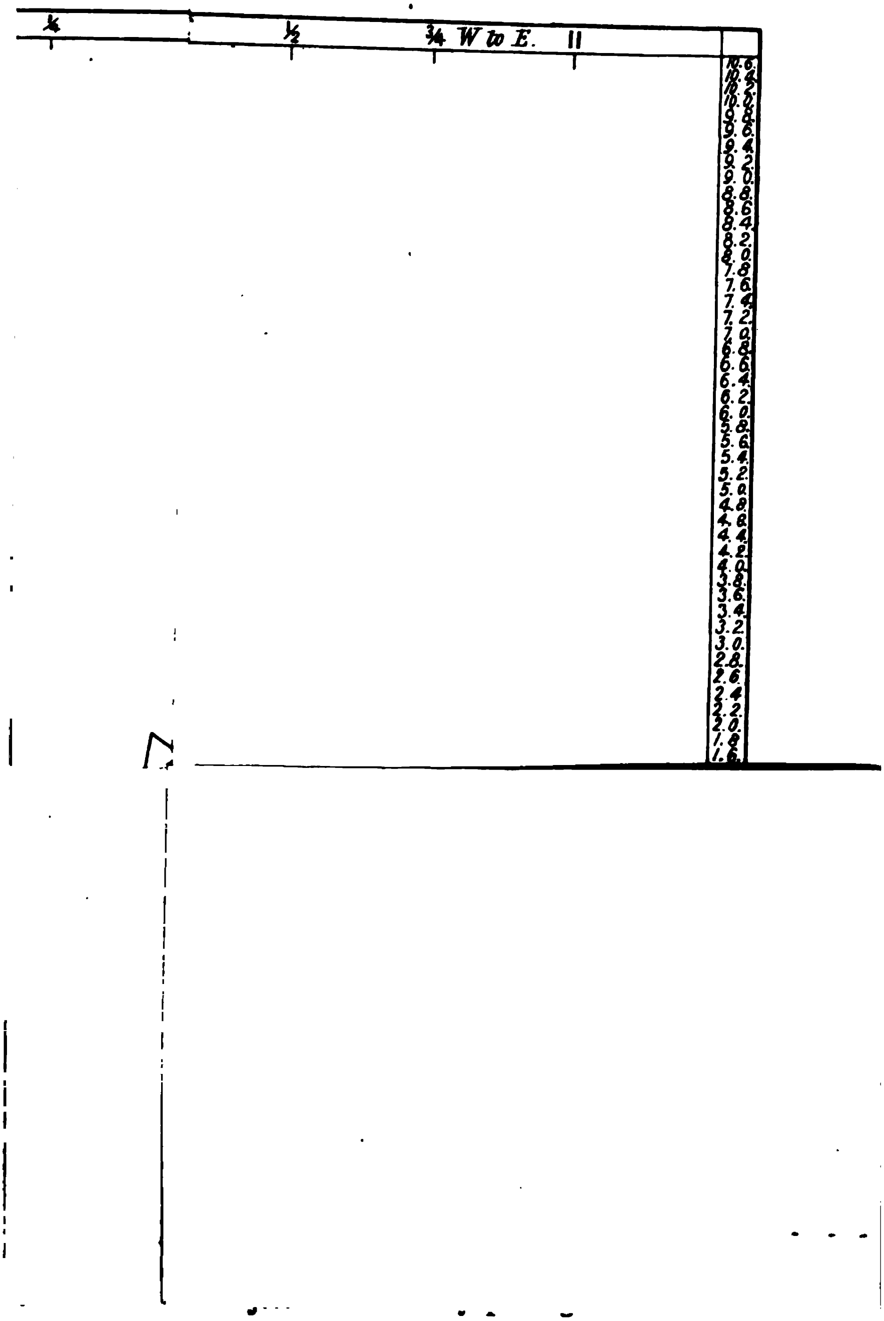
By E. O. WALKER, Member.

Observations of the earth currents of the line, Belgaum to Vingorla, were made from the 21st August to 8th October last, 1882. The line, a single one, 70 miles in length, consists of 63 miles of No. 5½ B.W.G. wire and 7 miles of No. 9½ B.W.G., supported on Prussian pattern insulators. Belgaum is situated on the Deccan plateau, 2,500 feet above the sea level; Vingorla on the sea coast, and the line lies about east and west. The earth connections were iron tubes at a distance from the ordinary earths. Some five hundred readings were taken when the line was disengaged. The general rules are that there is a zero between 7 and 8 a.m.; the current then begins to flow from Belgaum to Vingorla, east to west, attaining a maximum of 0·3 to 0·98 (average 0·595) milliweber between 9.30 and 10 a.m. It then decreases to a zero between 12.20 and 12.50 p.m., then begins to flow from Vingorla to Belgaum, west to east, attaining a maximum of 0·24 to 1·23 (average 0·58) milliwebers between 2 p.m. and 3.45 p.m.

These zeros and maxima almost invariably occur within these times. After 3.45 p.m. the current still keeps in the direction Vingorla to Belgaum, until a zero is reached between 11 p.m. and midnight, though indications of currents after 9 p.m. are small. From midnight to 5 a.m. the current seems variable in direction and small in amount; after 5 a.m. and up to the time of zero (between 7 and 8 a.m.) it flows from Vingorla to Belgaum. I have closely compared the readings at full moon and new moon, and at intermediate states, but find no reason for supposing that the currents are controlled, or variations created, by the moon. In Mr. Adams' admirable paper, read before the Society on February 10th, 1881, it was shown that there were daily variations between London and Cardiff following the movements of the moon, but it is a curious fact that the time of change and of occurrence of maxima do not by any means shift about at Belgaum as they are shown to do in London. It is also remarkable that the maximum current observed there was 0·104 milliweber, while in Belgaum it has been as much as 1·233 milliwebers, and has an average daily

MAGNE

E. O. Walker.



[illegible] 200 millimeters, and has an average d maximum in each direction of 0·6 milliweber. A theory has been held that earth currents in India are always in one direction: this idea may perhaps have arisen from the current of polarisation only being observed when testing the lines, or a current of polarisation combined with the ordinary earth current. However, my observations show most emphatically that there are regular changes of direction.

It seems plain that there is no single current running round the earth, but it would probably be found, if we had sufficient data, that places situated in the same meridian are subjected to tidal changes of current simultaneously.

In the discussion which followed Mr. Adams' paper, it was mentioned that the transference of a charge of electricity from one point to another on the earth's surface would induce currents in neighbouring suspended wires. If, then, there is a large induced stationary charge of electricity which remains fixed in position relative to the sun while the earth revolves, as it were, within this envelope, then it would be understood that there would be induced currents variable in amount and direction in telegraph lines on or near the earth's surface. The form which this static charge takes has, I think, yet to be determined. With more ample observations it may be possible to arrive at a curve or curves representing the induced charge by the times of occurrence of zero and maxima in telegraph lines. I have not a table at hand showing the diurnal changes in direction of the magnetic needle, but to the best of my memory the earth currents observed by me somewhat follow the needle.

The following paper was then read by the author:—

EARTH CURRENTS (THIRD PAPER), SUN-SPOTS, AND ELECTRIC STORMS.

By Alex. J. S. Adams, Associate.

It were an impertinence upon my part to attempt a detailed consideration of the physical constitution of the sun, but permit me, nevertheless, to recall one or two points that more particularly relate to the subject it will be my privilege hereafter to submit. By many of the earlier authorities, including Brewster, and at least the elder Herschel, the sun was considered to consist of an outer luminous atmosphere, an inner cloudy stratum, and a cool, solid centre or nucleus; whilst modern authorities are, upon the other hand, inclined to the belief that the nucleus is possessed of an amount of heat far in excess of the outer atmosphere. Be the precise nature of the nucleus what it may, we can safely conclude, from the unanimity of opinion upon the matter, that the solar orb consists of a nucleus, surrounded by an inner stratum and an outer atmosphere.

When the sun—*i.e.*, his outer atmosphere—is viewed through the telescope, it is observed to have frequently large and perfectly black spots upon it, sometimes so considerable as to be apparent to the unaided eye. These spots are neither permanent in form nor fixed as to position, but, when watched from day to day, and even from hour to hour, are seen to contract or enlarge, and otherwise to change form, as they gradually cross the sun's face from east to west.

In some instances—as in November last—a large spot will disappear round the sun's western limb, and, after passing behind the sun, will again appear at the eastern side, and cross the sun's face as before.

That these spots are gaps or openings in the sun's outer atmosphere is probable, but in how far the actual nucleus or body of the sun is exhibited through the openings it would be difficult to conjecture.

In passing to the consideration of electric disturbances upon our globe, you will be already aware that there is not only a weak, variable, unfelt, but constantly present natural electric force upon the telegraph systems throughout the globe, but that those systems are sometimes also severely deranged by temporary strong currents that ever and anon force themselves unawares upon the notice of the practical telegraphist. Such exceptional currents, designated in a former paper "storm currents," are by no means so infrequent as might at first sight be supposed. The fact is that minor storm currents are very frequent, although they are sufficiently strong to overpower the working currents of our telegraph lines only once, twice, or thrice in a twelvemonth, and sometimes during a whole year not at all. But occur when they may, these storm currents come suddenly, remain but for a short period, often for a few hours only, and then as suddenly vanish. What are these disturbers of the telegraphist's peace of mind? Whence come these vagrant forces? It will be my endeavour to offer for your consideration a reply to those questions.

In the communications I have already had the privilege to submit to the Society of Telegraph Engineers and of Electricians, it was my purpose, first to demonstrate the fact that the two forces, I mean the "earth current" and the "storm current," were distinctive, and should be separately considered, and my remarks were then entirely directed to a consideration of the normal "earth current" variations, the question of the "storm current" being purposely postponed; but the time has come when I would, with your kind permission, ask your attention for certain features and observations in connection with the latter force.

In attempting to develop a line of argument in reference to the production of natural electric currents, I was compelled to lay down some proposition upon which to work, and the following appeared reasonable:—

1. That the heavenly bodies are electrical.
2. That each body may be considered to possess a definite and invariable electrical quantity or quality, as regards the whole.
3. That each body exerts by *induction*, rather than by a simple discharge of electricity, an effect upon each of the remaining bodies, in proportion to the governing circumstances.

This appeared to be the logical outcome of reason, warranted by what is known through experiment; and based upon this, with the confirmation obtained from the observed variations of the normal earth current, I sought to show that the moon was one great disturber of the electrical condition of our globe, resulting in the formation of electric tides. It was further suggested that the natural electric currents might be set up by a simple retarding effect of the moon, whereby a difference of speed would be produced between the earth and her electricity, the strength of the resultant current being simply proportionate to the difference in their speeds of revolution. Given the earth and her electric revolving in equal ratio, there would be no natural current effe as we now have them.

Now, if the result of the disturbing influence of one *extern* body, such as the moon, be the production of a general and mo or less defined variation of current, should we not, conseque upon the earth's revolution, also expect a similar regularity in t effect of any other external disturbing power? and would not t fact of a similar tidal variation in any natural electric curre lead us to suspect in their case, also, an *external* source causation? And be it borne in mind, the fact remains that t effect of an electric storm is felt by all the telegraphic systems the globe—a fact that in itself is suggestive of external causatio

Such were the reflections that occurred to my mind reference to electric storms; but owing to the infrequency of t greater storms, the variableness of the currents and the want opportunity, it has been extremely difficult to follow up t variations.

During the great storm of August, 1880, a rough survey the current changes denoted, unmistakably, a regularity in t general rise and fall of the storm current and in its me direction, but further opportunity for more reliable observatio was awaited.

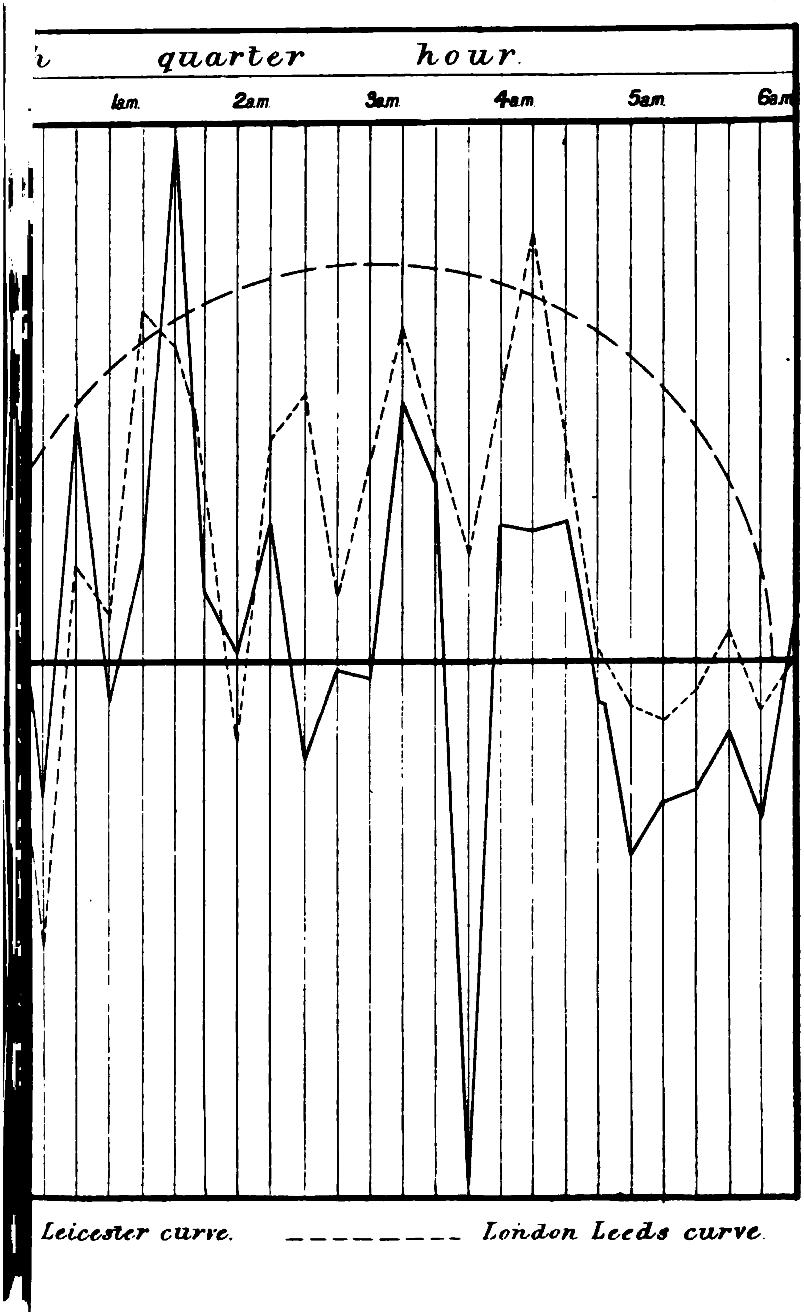
The great storm of November last presented such a opportunity, and the storm-current variations for many hou were obtained, the best result being from a wire extendin between London and Leicester, and for the result of which I a indebted to the kindness of Mr. W. H. Preece, F.R.S.

The observations illustrated by Fig. 1 consist of those taken upon the London-Leicester wire from 6 p.m. on the 17th to 7 a.m. on the 18th November, and a shorter curve of readings that were taken upon a Leeds-London wire between 11 p.m. on the 17th and 8 a.m. on the 18th, and from which it will seen how clear ar the indications of a double progression in the variation, similar t that which takes place in the normal earth-current variation—a progression which at once suggests a source of causation external to the globe.

RVATIONS, 17TH NOVEMBER, 1882.

A.J.S.Adams.

Fig 1.



But whence may we find that source? It has been the opinion of some, and of no gentleman more strongly than of Mr. W. H. Preece, F.R.S., that sun-spots were in some way responsible; but although, consequent upon the appearance of a disturbance to the sun's photosphere, an electric storm would sooner or later supervene, the storm current would be by no means simultaneous with the appearance of the sun-spot, nor would the sun-spot vanish with the disappearance of the storm current, and these apparent contradictions seemed to call for explanation before the theory could be regarded as satisfactory. That there was some sort of connection between solar spots and electric storms appeared evident, but how was the seeming discrepancy I have mentioned to be reconciled?

During the interval between the electric storms of last April and that of the following October, the subject of this apparent nonconformity of the current to sun-spot appearance and disappearance received careful consideration, and it ultimately occurred to me that the probability of the sun's electric influence proceeding from his solid opaque body, rather than from his luminous atmosphere, had not been sufficiently regarded—that in all probability the sun's atmosphere materially reduced, if it did not completely cut off, the sun's electrical forces:—a theory that requires no stretch of imagination, and which goes far to explain the comparatively slight electrical influence the sun normally exerts upon the earth.

Taking up the matter from this point of view, I was impelled to the conclusion that *the production of electric storm currents upon our globe is due to the greater or less exposure of a portion of the sun's solid body*, consequent upon the opening of the sun's luminous atmosphere, and to the direct action of that exposed portion of the sun's body upon the earth; and that, because force acts in straight lines, the sun's influence through the break in his atmosphere can only be felt by the earth when the exposed portion of the sun is actually presented to the earth (and it seems to me the precise constitution of that atmosphere, whether of self-luminous clouds or of circling meteoric systems, and the precise cause of disruption in that atmosphere, whether by local development or because of the entrance of some circl body into it as it traverses its orbital course, will not affect value of my argument)—a sufficient explanation of the curi want of conformity already alluded to.

It will be readily understood if, by way of explanation, I suppose the sun's atmosphere to be itself dense and dark, whil the sun's body is brilliant with light. The light that would pa from the sun's body through the atmospheric opening might b seen in the form of a distant ray, but only when the openin reached a point nearly opposite the earth should we get th direct effect of the light.

An opportunity for testing this aspect of the subject has bee recently offered. The electric storm of November was remarkabl my own records offering nothing in comparison with the strengt of its currents; moreover, the sun-spot was of considerable size frequently visible to the eye without artificial aid. For th precise dates of the sun-spot appearance and disappearance I have to thank Mr. William Ellis of the Greenwich Observatory, and it will be seen from Fig. 2, in which S represents the sun and his luminous shell, E the earth, and P a sun-spot, or cavity, that, if the sun-spot first appeared upon the eastern limb of the sun's disc upon a given date, by moving the spot round in conjunction with the sun's atmospheric period of revolution—say, one revolution in 25 days—the opening in the sun's atmosphere should centre upon the earth at about the sixth day afterwards, and that the first and last shock of direct electrical effect should be felt at nearly equal distances upon either side of that sixth day, the sun-spot disappearing about six days later still.

What are the actual facts in connection with this last November storm? The recent sun-spot became visible on November 12th, at the extreme eastern limb of the sun's disc, the earth current remaining undisturbed for no less than five days afterwards. Upon the morning of the 17th the electric storm burst upon us, and continued with varying vigour for three days, disappearing during the early morning of the 21st, after which the normal earth current assumed its wonted appearance. The sun-spot, however, remained visible until the 25th, when it

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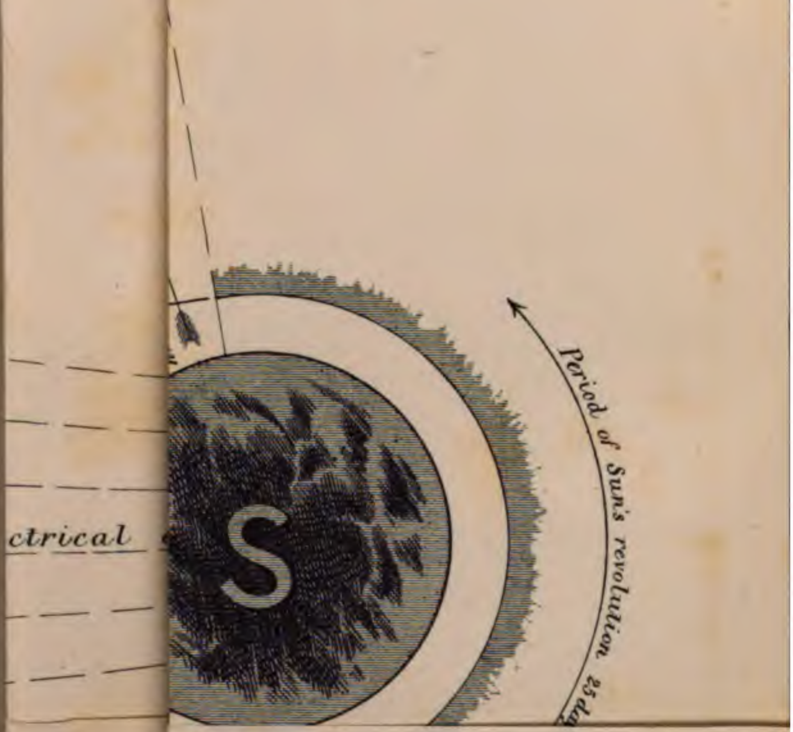
A.J.S.Adams.



disappeared over the western limb. Thus it would appear that the flash of electric influence occupied five days in reaching the earth by the sun's motion, and three days in passing the earth.

Upon the disappearance of the great spot round the sun's western limb, on November 25th, it occurred to me that the remains of that spot might show themselves upon the sun's face when the required time for their revolution round the sun had elapsed, and diligent watch was consequently made for the reappearance of storm currents. Upon the 15th and 16th December, dates at which the remains of the spot, if any existed, should be nearly at the centre of the sun's face, electric storm currents of very moderate strength were prevalent, and in answer to my enquiry, the Astronomer-Royal, to whose kindness I am much indebted, stated that "*the sun-spots observed on December 15th, etc., are, without doubt, the remains of the large spot of November* 12-25," and adds: "*Following the rotation of the sun, the November spot should have been on the limb about December 9th, at 7 p.m., and should be first seen here on the morning of December 10th.*"

Thus we see the remains of the November great spot appeared on the sun's eastern limb late on the 9th, followed by electric storm currents about six days later, and bearing out the idea that it is necessary for the exposed portion of the sun's body to get in a line with the earth through the opening in his atmosphere for the occurrence of an electrical disturbance.

I may also add that, whereas the great spot of April, 1882, was first visible on the 13th of that month, the electric storm did not really break upon us until the 17th, and prevailed to the 20th, a further indication of the correctness of the view I have sought to advance.

To me the facts connected with these electric storms are sublime to a degree, and altogether corroborative of the conjectured explanation—an explanation that, whilst amply vindicating the opinions of those who held the theory of sun-spot causation, also opens a wide field in the domain of practical astronomy. Nor is it too much to hope that the correctness of this view may be completely substantiated with the appearance of the next considerable solar spot, and its truth enable our astronomers to becom prophetic in fixing for us, days beforehand, the probable magn tude and time in appearance of electric storms.

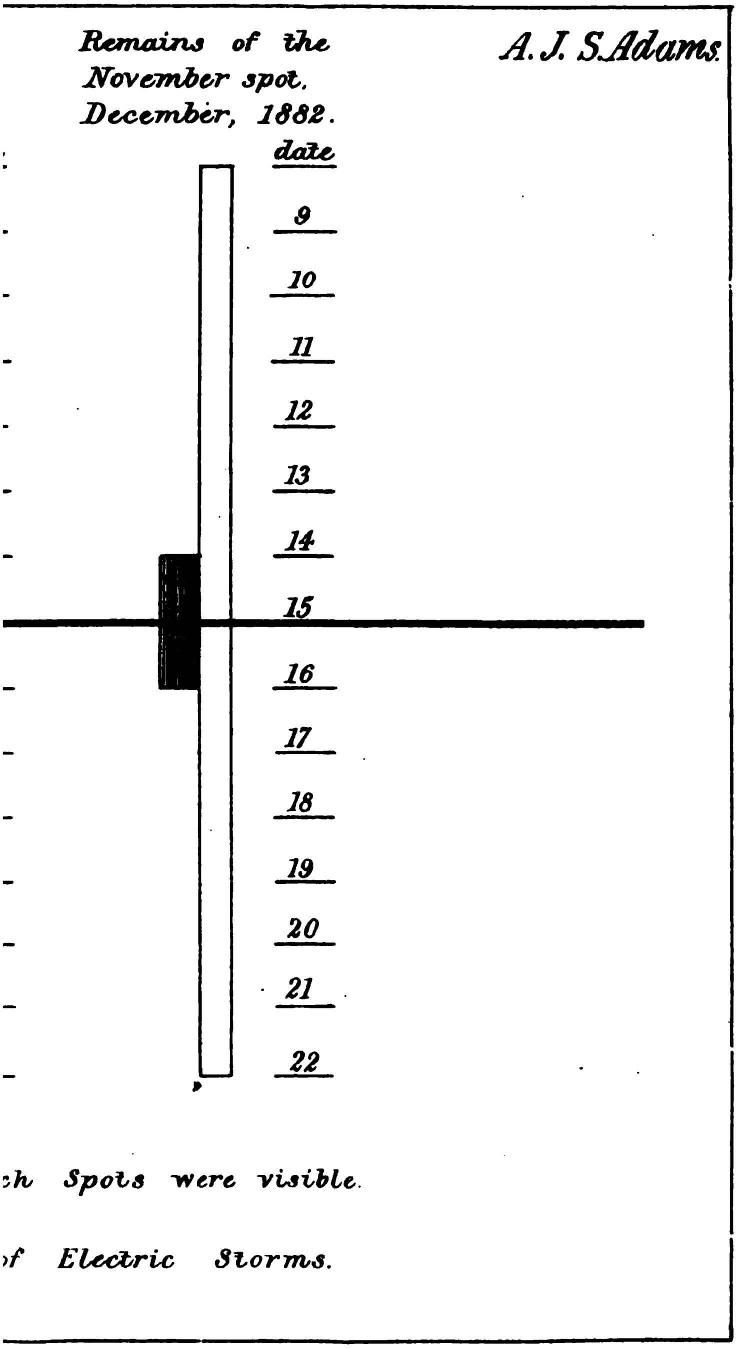
In conclusion, to my mind a feature of much interest occu in the question, What is the amount of force expended at th sun to furnish these electrical disturbances upon our earth? W must bear in mind that the force rays that through a solar sp strike the earth in a given moment of time form a small moiet of the whole that finds its way through the spot into space; an yet the moiety that did strike the earth was capable, in Novemb last, of producing a disturbance equal to no less than the $\frac{1}{20}$ of a ampère. Now, calculating back over the 95,000,000 of miles t the sun, we shall find that those few rays of force will equal at th unit distance of the sun's atmosphere 500,000 ampères nearly and it would be as interesting to know the consumption of co necessary per second of time for the production of those few forc rays, as it would how long the earth could retain its present constitution under the influence of the heat produced.

If, too, the hypothesis I have sought to advance regarding th effect of the sun's body through gaps in his atmosphere be established, the fact that such mighty forces are emitted by the sun nucleus is suggestive of a further feature in respect to the sol economy, and it is this: the earth, in her orbital course, is continually cutting lines of force, enfeebled though they be by th sun's atmosphere, and an electrical disturbance upon the eart with proportionate heat value, is the result. May not the cutting up and resolving into heat of the sun's electrical rays b the sun's atmosphere be sufficient to account for the otherwis amazing light and heat that are by that atmosphere thrown int space? Indeed, imagination fails in its attempt to grasp th mighty electrical forces which the sun, as a whole, may thro upon his atmosphere for conversion into heat and light.

28th November, 1882.

⁂ The suggested great difference in the electrical quality of the sun's bod as against that of his luminous atmosphere will hardly be considered too grea when it is remembered how vastly larger than the sun must be his out phere and the consequent decrease in its electric tension; moreover, it i

TIONS, 17TH NOVEMBER, 1882. *Fig III.*



probable that the sun's atmosphere partakes of a form not calculated to exhibit high electrical qualities. The exposure of the sun's body can at best be but partial, and owing to the variableness of the spot cavity the electrical forces evolved must necessarily be equally changeable.

The **President**: Gentlemen,—We have had four very interesting papers. In a Society like ours such a subject should produce a profitable discussion; therefore, if any gentleman present will favour us with his views on the subject, we shall be pleased to hear him. Perhaps Professor Adams has something to say on the matter.

Professor **W. Grylls Adams**, F.R.S.: I should have preferred to listen to the views of other members before joining in the discussion on Earth Currents, but, in obedience to the call of our President, I rise to open the discussion on this interesting subject, on which we have had three distinct papers this evening. In the discussion which followed the paper read by Mr. Adams some two years ago, on the regular diurnal earth currents, I stated a theory which had been put forward by Professor Rowland, and which is mentioned in Mr. E. O. Walker's paper. Professor Rowland showed that a static charge of electricity which is carried from one place to another will act in the same way in producing magnetic effects as a current of electricity flowing between the same two places—*i.e.*, a transfer of a static charge of electricity is equivalent to an electric current in producing magnetic effects. If I rightly understood his statement, Mr. Walker seems to think that a static charge which is fixed in position will be capable of producing magnetic effects. This is not the case; the magnetic effects are due to the transfer of the charge from one place to another.

With regard to Mr. Adams' papers and his theories, I should like to make a few remarks. In his paper read two years ago, he deals more particularly with the regular diurnal earth currents, which seem to depend upon the motion of the moon.

I am afraid that his theory cannot be accepted, viz., *that the earth has a charge of electricity upon it, the distribution of which is affected in a peculiar way by the position of the moon, and that these changes in the distribution give rise to the regular* *diurnal earth currents and terrestrial magnetic changes which are observed.* If we are to form a theory on a subject on which we have as yet comparatively few facts, I should still prefer to regard the theory which I threw out in the discussion two years ago, and in my paper before the British Association, as a more probable one, and one which is certainly in accordance with well-known principles of electricity.

The fact that the earth consists of magnetic matter, and that there are tides in the earth caused by the moon and by the sun —tides, I mean, not only in the seas, but in the solid body of the earth itself, altering its form, thus raising up a tide in that magnetic matter,—this fact would be quite sufficient to account for an earth current in a direction from the equator to the polar regions. Then, again, high up in the middle regions of the atmosphere, in the region of auroræ, you have a rare atmosphere which conducts electricity or allows discharges through it more readily than the lower and denser atmosphere. From the tidal retardation of this atmosphere of oxygen behind the magnetic matter of the earth caused by the attraction of the moon, we might expect a transfer of electricity in the atmosphere itself in a direction from the polar regions towards the equator: thus the attractions of the sun and moon on the magnetic matter of the earth and on its atmosphere might be expected to give rise to regular diurnal earth currents from the equator towards the poles, and a transfer of electricity in the shape of currents or electrical discharges in the higher regions of the atmosphere in a direction from the poles towards the equator; at any rate there would be nothing contrary to well-known principles in such a supposition. It gives a direct relation between the regular diurnal earth currents, magnetic disturbances, and the streamers of the aurora, which is intimately connected with them.

In his paper this evening, Mr. Adams has dealt more particularly with powerful earth currents and with violent magnetic disturbances, such as we have had in September and November last and during the past month. In my paper before the British Association, I compared such magnetic disturbances at observatories all over the globe—several in the northern hemisphere and some in the southern hemisphere. The photographic records taken at those observatories were reduced to the same time-scale, were placed over one another, and then the tracings compared with one another. I found that those magnetic disturbances occurred, as nearly as one can say, at the same instant all over the globe. There has been a general impression that these disturbances are larger in the neighbourhood of the poles than near the equator. A careful comparison of the magnetic curves shows that there is not such a very great difference in the magnitude of these disturbances, whether they occur in the north of Europe, or in Asia, or near the equator. Unfortunately, the instruments used in different magnetic observatories are not equally sensitive, and so a false impression may be obtained from the direct comparison of the curves unless the scale values are taken into account. Thus the instruments at St. Petersburg are very sensitive, at Vienna and at Kew and Melbourne they are less sensitive, whilst at Bombay and at the Mauritius the magnetic needles have not been magnetised to the same degree, and consequently the disturbances appear to be very much smaller.

Mr. Adams has referred to the earth-current disturbances of August, 1880. I have compared the photographic records of the magnetic disturbances of that storm taken at Toronto, St. Petersburg, Vienna, Kew, Lisbon, China, the Mauritius, and Melbourne. At all those stations—*i.e.*, practically all over the globe—the storm began at the same instant, and at stations even wide apart from one another the general character of the disturbances was the same. Thus at the beginning of the storm the vertical-force curves at Lisbon, where the vertical-force needle is very sensitive, and also at an observatory at Zi Ka Wei, near Shanghai, in China, where there is also a very sensitive vertical-force needle, the two curves, when put over one another, were almost absolutely identical, showing that the disturbance had begun at both places at the very same instant, and that this disturbance was absolutely of the same form at the two stations and taking place at the same rate —that in fact for a certain period there was no difference whatever in the character of those disturbances. Now, unfortunately, at most of the other stations, except St. Petersburg, the vertical-force curves do not appear to show great changes, because the vertical-force needles are not set up in a sufficiently sensitive state to get very much out of them. In fact, ordinarily the trace is simply a straight line, or as nearly as possible a straight line, with no variation except when a magnetic storm occurs.

The horizontal force curves are also similar in character at such widely distant stations as St. Petersburg, Vienna, Kew, Lisbon, Bombay, Melbourne, and Zi Ka Wei in China.

With regard to the theory that Mr. Adams brings forward, I am afraid that we have hardly sufficient facts established yet to form the basis for a theory which would be of very much service to us. We have as yet to arrange and compare the observations of magnetic storms made in magnetic observatories with the observations of earth currents (which have, of course, a direct connection with these magnetic storms—there is no question whatever about that) and with observations of auroræ, which have not been touched upon to-night, but which are most intimately connected with these magnetic storms and with earth currents.

In the present state of our knowledge we should be careful how we put forward theories, lest they should be found to be based entirely upon conjecture. For instance, I think the theory which Mr. Adams has just put forward with regard to the electrical influences coming from the body of the sun in straight lines through a spot, and only affecting the earth when those straight lines happen to fall upon it, scarcely agrees with the established principles of electrical induction, and will hardly account for the facts observed, viz., that after the appearance of the sun-spot round the edge of the sun it will be four or five days before the magnetic disturbance, which appears to be connected with that sun-spot, begins to affect the earth. I think we must see, first of all, a little more connection between this disturbance on the sun's surface and what we know of electrical induction. The sun has an atmosphere in a very disturbed state, and we know a great deal about this atmosphere. We know the character of the motion in the neighbourhood of a sun-spot from the spectra which have been observed—that in fact a sun-spot is a sort of tornado or whirlpool, the magnitude of which we have no conception of, but the character of which we know very well from similar disturbances produced in the atmosphere of the earth or in the ocean. We may say that we have a very large whirlpool in the atmosphere of the sun. But I think that we should not jump to the conclusion that the sun's atmosphere is a shield to protect the earth from the electrical influence arising from the body of the sun, even supposing that the body of the sun were electrically charged. If there is an electric charge on the body of the sun, surely it will be on the outside of the solar atmosphere that we should get this tremendous charge of electricity, and therefore you may say that the earth would be entirely open to its influence. Electricity spreads to the surface, and therefore we may expect the outside of the solar atmosphere to be charged up to a very high potential, and so we should not look for the disturbance to come through a rift in that atmosphere.

Of course it is necessary to bring to bear on this subject all the evidence we can, from spectrum analysis and from other sources, so as to find out really what connection there may be between sun-spots and the magnetic disturbances on the earth. I think we must regard the sun-spots rather as signs of very great disturbance in the solar atmosphere, than as holes through which electrical influence may pass.

Observations seem to show that when the sun's atmosphere is in a state of very great disturbance there is less heat received by the earth from the sun, and hence the conjecture has been thrown out that this diminution in the supply of heat to the earth is the cause of the aurora and earth currents and magnetic disturbances. Again, it has been suggested that the sun is a magnet, and has a direct magnetic influence upon the earth, and that these magnetic storms may be due to changes in the earth's magnetism which are caused by magnetic changes in the sun. We must trust that future observations will clear our ideas with regard to earth currents, auroræ, and magnetic disturbances.

Mr. C. A. Morgan: I have given the question of earth currents considerable attention, not only since the great magnetic storm of November last, but for some years past. When Mr. C. F. Varley was engineer to the Electric and International Telegraph Company, I remember a magnetic storm stopping our communication. That was twenty-four years ago.

I will not venture to suggest a theory after what Professor Adams has just said; but it has occurred to me that there is a little electrical instrument which I am not aware has ever been mentioned in this room since earth currents have been talked about (although they were spoken about at some of the first meetings of our Society), and that is "Delezenne's circle," which, when revolved in such a manner as to cut the magnetic lines of force, gave readings upon a galvanometer. During the discussion on Mr. Adams' last paper, Professor Adams referred to a remark of Professor Clerk Maxwell, who said, that if a wire was suspended from the poles to the equator round the earth, a current would be obtained in that wire. Now, quite independently of that, as well as of the subsequent remarks made by Mr. Banker on the same paper, I formed a theory in my own mind which "Delezenne's circle" seemed to confirm. It does seem rather strange that the general flow of earth currents, both in England and in India, are, as a rule, in one direction. Schwendler, in his very instructive paper read before the Asiatic Society, shows that in India the facts are as I state;[*] and Professor Adams has just suggested the conjecture that it may be the result of a flow of electricity passing from the equator to the pole on the surface of the earth, or *vice versa*, and from the equator through the upper regions of the atmosphere back again.

In answer to Mr. Adams, as to the amount of heat received by the earth from the sun, we shall have to step beyond the region of telegraphy to that of physical geography and astronomy. Geographers (Pouillet, Herschel, and Maury) tell us that the amount of heat given out by the sun in one day to the earth is sufficient to raise a caldron of water as large as the Principality of Wales, and a mile in height, from the zero point up to that of boiling.

Returning to Professor Adams' remark as to the supposed currents from the equator to the poles, and passing both in the lower and upper regions, it is well known that a very large constituent of the atmosphere, viz., one-fifth, is oxygen, which Faraday demonstrated to be magnetic. He has also shown that as the temperature of oxygen is raised its paramagnetic force diminishes, being resumed as the temperature falls again. He says: "These properties it carries into the atmosphere, so that the latter is, in reality, a magnetic medium, ever varying, from the influence of natural circumstances, in its magnetic power. If a mass of air be cooled, it becomes more paramagnetic; if heated, it becomes less paramagnetic (or diamagnetic), as compared with the air in a mean or normal condition."* The earth revolves at a very great speed, and the whole of the atmosphere is influenced by its revolution. Now, is it possible that the rotation which the earth has is sufficient to cause these invisible lines of force, which, with the atmosphere, are being acted upon by the force of gravitation, to take up a certain position due to part of the atmosphere being oxygen, and so forming, as it were, a magnetic field which sets up induced currents of electricity in the wires? When there is no sun-spot present there is always a normal earth current, but when the sun-spot comes opposite our earth, and we are in the sun-spot shadow, then something peculiar takes place by which every telegraph wire is stopped for a time. Is that caused by the fact that the magnetic equilibrium, if I may term it, of the earth is disturbed, and these lines of force, which up to that time have behaved symmetrically, are, as it were, stopped or caused to deviate, and, although the earth continues its previous motion with the wires upon it, they cut other innumerable lines of force made by the presence of the sun-spot, and the so-called earth current observed be set up? It is a fact that when a wire is looped the deflection is lost. But it is not necessary to make the loop at both ends with wires going geographically parallel. On the morning of the first appearance of the November earth currents, a curious case came to my notice. I made a loop between London and one of the Liverpool offices, and told that office to "loop," but he did not do so. I made the loop myself. After the circuit had been working for a considerable time, Liverpool said the earth currents had disappeared: "Please put the wire straight." Now, as I had made the loop, I replied that it was not necessary for him to do so at his end (the earth currents were as strong as ever). This was on a Wheatstone circuit, and, although the two wires at the distant end were making earth at the earth-plate common to both, it made no alteration upon our working wire. Working was carried on in one direction on a Wheatstone wire to Manchester, unlooped, and, although the prevalent earth currents disturbed many circuits, the one I refer to was able to do good work.

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[*] *Journal of the Society of Telegraph Engineers*, Vol. III., p. 175.

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* *Phil. Mag. and Journal of Science*, 4th Series, No. 1, January, 1851, p. 73.

I should like to ask telegraphers a question that has been put: Is it possible in America, or anywhere else where double current telegraphy is used, to work a circuit duplex during the prevalence of earth currents, magnetic storms, or sun-spots? The answer to such a question would be very interesting. Some circuits did actually work duplex on the second day when earth currents affected other single-working unlooped circuits. This point can, I think, be explained by assuming that the earth current obtained was not stronger than the working current which was used, in which case a balance of the circuit might be maintained without detriment to the work, and I think, also, that it is possible for such a circuit to be less affected than a direct working or single circuit not using differential apparatus.

I have perhaps gone out of the way in speaking of physical geography, but I think that this question in particular can only be answered by physical geographers; and when it has been before our meetings much has frequently been said of the observations that might be made by telegraphers to advance science in general. I want, if I may be allowed, to drop a hint as to the equipment of all the observatories in the world, viz., that every observatory throughout the world (I believe it is already the case at Greenwich) should be supplied with a short telegraph line, and also to have a recording instrument showing the normal earth current every day. It could be done at little expense comparatively. In certain parts of the world the wire might be laid along the ground, at other parts it might be suspended on poles, and I believe by that means better results might be obtained than can possibly be given by telegraphers, because, when the earth currents appear in the manner in which they did the other day, it is utterly impossible for any telegrapher to give his attention to a matter of that sort at busy stations.

Professor McLeod: May I be allowed to make a few remarks with reference to the theory of sun-spots. The explanation that we have heard with regard to the solar nucleus being seen through the sun-spot is, I think, one of those old theories which by this time have been almost exploded. It appears that these solar spots are places where the gases are cooler than in other portions of the sun's photosphere. In the neighbourhood of the spots there is almost invariably seen a bright patch, called a facula, which appears to be an uprush of hot gases from the neighbourhood of the spot. These hot gases, in cooling, descend and produce those phenomena that are called sun-spots; and on examining the sun-spot with the spectroscope we find that the gas in the spot itself has a lower temperature than the surrounding atmosphere.

There is one very curious phenomenon observed in the sun-spot which is not explained by this theory—I mean the very sharp line of demarcation between the nucleus of the spot and the penumbra. It is very difficult indeed to explain what is the actual cause of this: we cannot well say that it is the nucleus of the sun, because there is no proof that it is so, and it is not easy to see how the descent of cold gas will produce this sharp line of demarcation. If the nucleus of the sun were the cause of the disturbances which manifest themselves, they should begin shortly after the spot appears on the eastern limb of the sun, and continue almost until the spot disappears on the western limb. Mr. Whipple, of the Kew Observatory, pointed out to me the other day the curious fact, that it is only while the sun-spots are in a certain position that the disturbances take place: they do not take place during the whole time of the appearance of the spot, but only when the spot stands in certain positions of the sun with regard to the earth.

Now, the faculæ—these bright spots which are seen in the neighbourhood of the dark ones—are shown to be incandescent gases driven off from the sun with very great velocity. Gases have been seen to be projected from the sun at the rate of 150 miles in a second; and one can very well understand that an electrified body travelling at such a rate towards the earth is very likely, indeed, to produce some kind of disturbance in telegraph wires. I do not say that this is so, but it is not improbable.

There was one very remarkable observation made on Sept. 1st, 1859, by Mr. Carrington, who was watching a large solar spot. During his observation two bright spots were seen at the edges of the dark one, which were visible for five minutes, during which time they travelled ·35,000 miles. At the same time the instruments at Kew showed a magnetic disturbance which continued as long as the bright spots were visible. Now I think this is in itself almost sufficient to prove that the effect which is known on the earth as earth currents, or magnetic disturbances, is not produced by the nucleus of the sun, but by something above the surface of the photosphere, for the bright spots in Carrington's observation were above the spot, and probably above the surface of the photosphere.

Professor J. PERRY: No work of mine has been devoted to the question of the sun's static induction and its effects since I spoke here last on the subject, in the discussion which followed Mr. Adams' second paper. My remarks may therefore be given very shortly. I would first call attention to the fact which has not, I think, been sufficiently recognised yet by the meeting—that it is a very good thing indeed that these earth currents should be mapped out so carefully by the only people, probably, who have a chance of doing it systematically and properly; and I should also like to say that, instead of one telegraph wire being put up at every observatory, it would be well to have two telegraph wires placed at right angles to one another. The only other remark I would make is about an inaccuracy in Mr. Adams' diagram, which, although probably of no moment to ordinary members of the meeting, may have some evil effect on students and the younger members. Leaving out of account the consideration as to whether it is the dark nucleus of the sun or some electrical protuberance from the sun which may produce electrical effects on the earth, it is perfectly certain that as the figure is drawn, it is quite impossible for the exposed portion of nucleus to have a large static inductive effect on the earth. We all know that any electrified body is quite incapable of influencing outside bodies or of being influenced by them, if it is covered by a conductor. The idea involved in this figure is, of course, that the sun is surrounded by such a conductor, and that in this conductor there are spaces. It is certain that if the conductor and the nucleus and the space were of the proportions which we see in the figure, the shading action would be almost perfect at a distance of a million miles from the sun. We all know that a network of thin wire completely protects the Thomson electrometer from the static induction of outside bodies.

I have no doubt that if the nucleus of the sun in the figure were made larger than shown, and if the distance between the conductor and the nucleus were made very small, then the nucleus at the spot might have the action or some such action as is spoken of. It is, however, misleading to indicate that inductive action by the dotted lines shown on the figure.

Mr. C. A. MORGAN: I intended that the suggestion I made should cover two telegraph wires placed at right angles to each other, similar in fact to the existing arrangement at Greenwich. The Astronomer-Royal and Mr. Glaisher have called attention to the fact that the difference between the deflections observed on the declinometer joined to the Greenwich and Croydon wires were far more interesting than observations recorded by the ordinary declinometer, and also on that instrument which was joined to the wire extending in the other direction to Dartford.

Mr. H. R. KEMPE: I think it will be generally admitted that the more observations we can get on subjects like earth currents the better. But I think that it is very necessary that the observations be not made in the manner indicated by the diagrams before us. I observe that in all cases the observations are taken at definite intervals of time. Now, what proof have we that, if each observation were taken a few minutes later than was actually the case, the shape of the curve would not be entirely altered? It seems to me that the observations, to be strictly useful, should be taken by the photographic process in which the curve is absolutely continuous.

Lieut. A. H. **Bagnold**, R.E.: A remark has been made with reference to Mr. Adams' paper, that we should study physical geography in connection with storm currents. I wish to echo that opinion, for I fancy that in that paper and the subsequent discussion on it we have been endeavouring to arrive at a general theory of storm currents from observations solely on the telegraph lines of Great Britain, whose climate is temperate and equable.

In 1880, I had charge of the telegraph line between Newcastle, in Natal, and Pretoria, in the Transvaal, a distance of 200 miles. During the clear, dry winter months no earth currents were noticeable, but during the spring and summer months our line was generally unworkable for the best part of three days per week, owing to the presence of powerful storm currents. These currents, as a rule, accompanied either snow, hail, wind, rain, or thunder, or some combination of such storms. Apart from heavy line discharges, coincident with flashes of lightning, the currents varied in strength and direction according to the position and distance of the centre of atmospheric disturbance, the path of the storm generally passing down our line from south to north.

Unfortunately, I had no apparatus for electrical measurement other than ordinary detectors, the needles of which were often deflected to the full extent of the scale for many minutes together; and, as I have stated in a previous paper, on one occasion, during a heavy snowstorm, the end of the Newcastle-Standerton wire (83 miles) at Standerton was, when held in the hand, so hot that it had to be dropped.

Now, I have been told that all this was due entirely to atmospheric electricity, and has nothing to do with so-called earth currents. My impression is that we cannot disconnect atmospheric electricity from the electrical disturbances on the earth's surface; and I fancy that, were we to obtain simultaneous and reliable meteorological and electrical reports from stations at various and well-selected points of the globe, we should arrive at some general theory of the following kind:—

Accounting for the normal flow of electricity over the surface of the earth, or, in other words, the earth's magnetism, by the mere fact of its continuous revolution and its exposure to the radiant heat of the sun, it follows that any climatic interference with that radiated heat will cause disturbances in the potential of the earth.

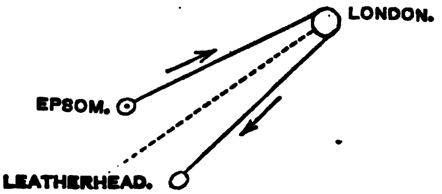
Diurnal disturbances would be caused by diurnal climatic variation, and storm currents by extraordinary variations, such as the condensation and accumulation of enormous banks of clouds.

Storms in the sun's atmosphere must of course interfere with the radiation of the sun's heat, and therefore may rightly be considered as one cause of storm currents.

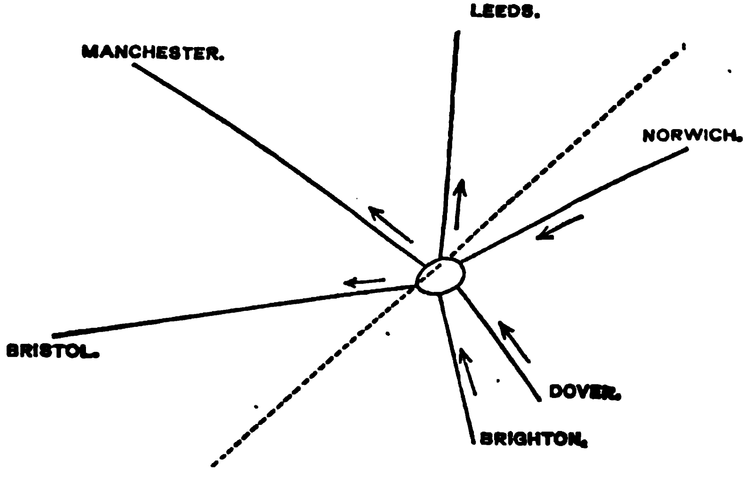
If, however, it were possible for us to take up a position midway in space, and establish proper communication, I fancy we should find that storm currents on the earth would be found to be due more to *earth-spots* than sun-spots.

Mr. W. H. **Preece**, F.R.S.: The answer to be given to Capt. Bagnold is simply to say that we should be only too glad if members of the Society from other parts of the world would favour us with communications on this interesting subject; and I think that the phenomena to which he alludes are rather more due to atmospheric electricity than to earth currents. We are very thankful for what we receive, and we are very much obliged to those gentlemen who have written the papers read to-night. Two of those papers were from India, and I also have received a letter from India, from Mr. Luke, confirming very much the observations made by Mr. Walker, and showing one fact to which I want to allude. You all know that I have devoted a good deal of attention to this subject of earth currents. It is now exactly 28 years since I made my first appearance with a scientific article. That article was written on "Deflections," as we then called them, and appeared in a magazine that was edited by our treasurer, Mr. Edward Graves; and it is singular that, during the interval from then (1855) until now, information or knowledge as regards earth currents has but little advanced. The facts that have been particularly brought to our notice are very few. We know that earth currents are associated with magnetic storms, and in fact the movements of the needles at our magnetic observations are a faithful record of the earth currents on our wires; so accurate are they that we can almost with certainty take the indications of the magnets as an indication of what takes place on the wires. Again, we know that these currents and storms are intimately associated with sun-spots, so much so that those peculiar periods of maxima and minima that are associated with sun-spots are equally associated with earth currents. In the year 1872 we had just such a period as we are passing through now: in the year 1859-60 we had just such another. Every 11 years or so we have these periods of maxima, with intermediate periods of minima, and not only is this the case with sun-spots, but with auroræ, with our own earth currents; and some people go so far as to say with potato disease, famines, etc. Again, there is one very great fact that is coming more and more into prominence now, owing to the tremendous and rapid extension of telegraphy all over the world, and that is that these phenomena are not confined to little places on the earth like England, Ireland, and Scotland, but that they take place simultaneously all over the globe; and, as Professor Adams has pointed out with respect to magnetic storms, so with respect to earth currents. We find, for instance, on November 17th last, we had one of the most terrific earth-current storms that we have ever experienced; and we find that it was felt simultaneously all over the globe with equal force and with equal peculiarities: so that we may say these effects are cosmical, they are simultaneous all over the face of the earth, and everywhere they are distinguished by precisely the same peculiarities.

There can be no doubt, although it has been doubted in this room, that there is an intimate connection between sun-spots and earth currents. I have an idea that I have heard even Mr. Adams himself contest my theory on this point, and I look upon his paper to-night as a species of conversion on his part to the views that I have so long advocated. The great fact that taught me the connection between the two was that mentioned by Professor McLeod, viz., the sudden appearance in one of those sun-spots of a bright spot than ran across the dark space—an effect which was observed simultaneously by two well-known astronomers at two different places. Professor McLeod said, that simultaneously with the passage of this spot a disturbance was observed at Kew; so also simultaneously there was a tremendous disturbance of the telegraph wires, which was noticed not only in this country, but in other places, and in Norway the current generated in the telegraph wires was so strong that it burst up one of the telegraph instruments at Christiania. The main features that we know I have mentioned, but many more facts are wanted to produce some satisfactory theory. It is always very easy to produce a theory: it is very very difficult to substantiate that theory. I have been guilty myself of trying to establish a theory, and have given it up in despair; for the more one tries to make the theory agree with facts observed, the more one finds oneself floundering in a mess. There is one fact that is altogether to my idea opposed to Professor Adams' notion. He alluded to the probability of there being a current flowing through the crust of the earth and flowing back through the atmosphere; but we have one great fact that was brought most prominently before us at the great storm of November 17th last, and that was that it was characterised by the most rapid and most wonderful reversals. Currents were flowing in the positive direction, in some places acquiring a strength of not 50 milli-ampères, as Mr. Adams mentioned, but 100 milli-ampères in strength; and these powerful currents instantly, in a second, were completely reversed in direction, not only once, but many times. To watch the needles on that morning of November 17th was excessively interesting. It was as though there were tremendous palpitations passing through the crust of the earth, reversing each other with wonderful rapidity, and devoid of that steady deflection that generally is characteristic of earth currents. There were two or three very interesting facts brought out in that storm, but unfortunately, as Mr. Morgan pointed out, the attention of all telegraphists is so wrapped up in their work at the moment that they cannot take scientific observations that can be worked into any shape or form. I have a pile of papers about the storm of November 17th, but it has been almost impossible to deduce from these thousands of observations anything of value. But one peculiar effect is very interesting. Two wires are provided for the telegraphic service between London, Epsom, and Leatherhead: geographically they are situated as shown below:—



The earth currents were observed on these two wires (Epsom and Leatherhead being only some six miles apart), and the direction of the earth current on the wire from Leatherhead to London was positive in its direction, while on the wire from London to Epsom it was exactly the opposite, so that we had, in two wires running nearly parallel to each other, the singular fact that the earth current was reversed in direction. The conclusion one would first arrive at from this would be that some mistake had been made in the observation; that probably the needle was reversed, or something of that kind; but the most careful pains were taken to verify the observation made by Mr. F. Morgan, and there is no doubt that it was strictly correct. It is very easily accounted for; and the very fact of its being accounted for leads one to the point I want to reach, and that is, how very necessary it is to find those points where a change of current takes place. Epsom and Leatherhead are joined by a wire through London. The current would flow from London and go back, which would show that there was a difference of potential between the two points; but it is necessary to account for the fact that the potential at London is less than it is at Leatherhead and greater than it is at Epsom; and the result shows that the line of equal potential passed through London between Epsom and Leatherhead, as indicated by the dotted line. One main fact that I am most anxious to get out in the observations of earth currents is, what is the line of least disturbance, and what is the line of greatest disturbance. In the case I have just mentioned, that fact comes out, and if it could be arrived at in other observations it would be most useful. These observations were made about 10 a.m. on the 17th November. At about 4.15 p.m. on the same day similar observations were made on wires serving Brighton, Exeter, Swansea, Liverpool, Leeds, Colchester, and Margate, and at that time the line of maximum current had turned round 90°, or almost at right angles to the maximum disturbance at 10 a.m. Again, observations made at Newcastle-on-Tyne, at 10 p.m. to 11 p.m. on the same day, or about twelve hours after the morning observations, showed that the greatest disturbance had exactly reversed itself, and these results show that at 10 a.m., 4 p.m., and 10 p.m., the line of greatest disturbance pointed towards the sun, or very nearly so. By those three observations we have one great fact brought out which I have so constantly urged, and that is that there is an intimate connection between the direction of these earth currents and the direction of the sun. I should like to call the attention of observers to the necessity of great care in recording the direction of earth currents. Suppose a station as below, like London, serving several towns by wires radiating from it, if



observations are taken on one wire, say, that serving Brighton, the customary plan is to call that current positive which is the same in direction as that which would be received from the copper pole of a battery placed at Brighton, and it would be in the direction shown by the arrow, and called by London a positive current. If a current were also received by London from Norwich, it would go in the direction shown, and would also be called by

London a positive current. Now, take the Leeds wire, and suppose an observer receiving currents there, he would find that he got the same current as he would get from the zinc pole of the battery, and he would call it a negative current, and would rightly indicate it so. Supposing from Bristol, London had a zinc current, and also from Manchester he had a zinc current, and then from Bristol he had the same, but from, say, Dover a positive current: these operations would show that there must be a line of least disturbance somewhere near that indicated by the dotted line; but it would also show that the currents flowing north of London and south of London were precisely the same as regards direction, only by the curious convention that we have introduced in telegraphy, although the currents flow in exactly the same direction and are the same currents, yet in the one case we call them positive, and in the other case we call them negative. This leads to a great deal of confusion. It is a point on which I think, one of these days, our Society will have to lay down some rule. Some rule should be established to indicate that earth-current observations are positive with respect either to the equator, or to the axis of the earth, or to the meridian, or to something quite independent of the position in which the observations are made. For instance, if we assert that whenever there is a difference of potential which shall be higher to the south than to the north, it would mean a current would always flow from the south to the north along the meridian of Greenwich; and, if we laid down a convention that that should be always positive, we might get accordant results. At the present moment there are thousands of observations which I have pored through to no practical result whatever, because it is quite impossible for me to eliminate positive currents from the negative currents. There are several theories on this matter. I have abandoned theory myself, and want facts, and if we could only induce other observatories besides Greenwich to have these cross wires (or Greenwich having these cross wires), and send their observations here, we could do a great deal of good for our service. But the real fact is, that Greenwich has had these cross wires for the last 20 years, and I have never, neither do I think there is any one in this room who has, gained one single useful fact from the possession of those wires that are maintained for the use of Greenwich Observatory. I have kept you rather too long on this point, but I think that if we can only succeed in inducing telegraphists to make observations as often as they can, and make them accurately; if we can induce Professor McLeod, for instance, at the East Indian College, Cooper's Hill, to run cross wires and take observations; and induce Greenwich to send us their observations —then I think in another year or two we shall have reached the region of theory and be able to promulgate to the world a result more than the world knows now. It is a remarkable fact that last year there was an offshoot of the Congress of 1881 held in Paris. One of the subjects before it was that of earth currents, and I am sorry to be obliged to announce here that, after reading all the discussion that took place at that Congress on earth currents, I was surprised to find that there was no one who attended that Congress that knew anything whatever about our present knowledge of earth currents.

Professor J. **PERRY**: Might I say, in connection with the very interesting remarks made by Mr. Preece, that it would be well, in observing earth currents, for us to get, not merely the current in webers (or rather ampères) or milliwebers, but the difference of potential between the two ends of the wire, and a statement as to which is of the higher potential. If we are told that a current is 50 milliwebers, we really get no information as to the difference of potential at the ends of the wire unless the resistance of the circuit is also stated. Now, I understand from Mr. Kempe that the resistances of the circuits which are involved in the statements made to-night are really very different. Fifty milliwebers in one case would be a very different thing from 50 milliwebers in another case. If the differences of potential were stated in volts, and we were told which of the two places had the higher potential, then I think Mr. Preece would find that probably his conditions would be satisfied as to mapping out what occurs during a storm, because then we could draw the equipotential surfaces. The hour is very late, but I should like to ask, in regard to the experiments between Leatherhead and Epsom, whether any other town was taken; for, as London is so close to a large river, one might expect that any other place along the river would be on the same equipotential line. For the same reason I should hardly think that perhaps Greenwich or even Cooper's Hill are very good places for making these observations. I should rather take Derbyshire, or some other central part of England, for my cross wires, not near any large river which is likely to produce an equipotential line in the neighbourhood.

Mr. C. E. Spagnoletti: Following the remarks that have been made with regard to the storm of November 17th, I might perhaps say a word or two as to that portion of the country over which I have the opportunity of getting some information. We found that throughout the West of England and South Wales we had the greatest magnetic deflection or earth current that we have ever experienced. The telegraphs on the Great Western line were entirely stopped for some time, and so strong was the current that the signal bells, which require a good current to work them, were continually rung, and not only rung for a time, but intermittently for some time—that is, actual signals of a certain number of beats were given most distinctly, similar to a telegraph signal; but as these did not complete the whole operation of signalling, no confusion arose therefrom. In one case, when an instrument was pegged down, and the first I have heard of, the battery current was reversed by the strong earth current. Now, to reverse that current, the potential of the earth current must have been 15 to 20 volts, for a 12-cell Daniell battery or its equivalent is employed upon such a circuit, and to reverse its power must require more than double that amount of current. Intermittent-current instruments—the one-wire system—were useless during the times the deflection lasted.

I have noticed in these magnetic storms that the long circuits, as a rule, are more affected than the short ones, and that the effects are apparent on the long circuits after the short circuits cease to be affected; but the limit I find to that is three or four miles. The deflections were strong on the 17th and 18th, and very weak on the 19th, while they were again strong on the 20th. One circuit running from Reading to Birmingham was deflected to the right, and one from Reading to Swindon was also deflected to the right, but on two other circuits, one running north and south and the other east and west, the deflections were in a contrary direction. No effect appeared on short circuits or junctions from the railway into post offices, from ½ mile to ¾ mile in length. But I should like to know, if it can be pointed out, how it is that these very short circuits show such results, because it would seem, as Mr. Preece just now pointed out, that there is a difference of potential within a few miles from one place to another, therefore the potential must be varying all over the surface of the earth within one, two, or three miles. The only way I can account for it is that the charge of the earth is so excessively high that its own resistance intervenes and splits the current, the wires running parallel with the flow of the currents form simply additional channels for it to flow, and it divides itself in its course over the earth and any wires in its way capable of being made use of, whether long or short; but it would appear that wires running at right angles to the flow or course of the natural currents cause deflections in an opposite direction to those running parallel. I cannot think it possible that the deflection is due to difference of potential in so short a distance. I should mention that so strong was the current on the 17th of November that the paraffin papers of some lightning protectors were fused, and in one case two papers had accidentally been put between the plates of metal, and they were perforated by the discharge.

The **President**: Gentlemen,—At our last meeting I ventured an opinion that our knowledge of earth currents might be greatly extended if we could prevail on the Telegraph Companies to take simultaneous readings, if only for a short time, on their lines daily, and what I have heard this evening has much strengthened me in that opinion. The time is too far advanced for me to detain you to-night with any lengthy remarks of my own. Mr. Graves, in his paper, refers to tests made on the 1873 cable. Perhaps it may be well to mention that the cables belonging to the Anglo-American Telegraph Company are known by the year in which they were laid. When their first cable was laid in 1866, it was impossible to obtain any reliable data as to the electrical condition of the same at the Heart's Content end; for during the whole of the night a current flowed out of that end of the cable, of higher potential than that of the hundred cells applied for the insulation test, although at Valentia there were no electrical disturbances of any kind, and consequently reliable results were obtained. At Valentia the weather was very fine, but at Heart's Content very stormy with much rain. On the following day there were no signs of earth currents at either end, and the repeated tests showed that the disturbed condition of the Heart's Content end during the previous evening had in no way interfered with the tests taken at Valentia. Mr. Graves does not mention whether those strong currents he refers to were also noted at the other end of the cable.

The Atlantic cables are laid nearly due east and west, and they are frequently affected by earth currents, while the cables of the Brazilian Submarine Company running nearly due north and south are seldom affected, and then but slightly. But there is a cable, about 300 miles in length, laid in a north-east direction, which is seldom to be found free from very strong earth currents.

It is too late to detain you longer: I will therefore ask you to express your thanks to those gentlemen who have so kindly contributed the papers we have heard this evening, and more especially to Mr. Adams, who has gone so deeply into the subject.

This was cordially acknowledged.

A ballot then took place, at which the following were elected:—

As Foreign Members:

Frank Del Plain.
A. J. Gustin.
F. S. Van Valkenburgh.

As Associates:

Douglas Clavell Bate.
George Chatterton.
Charles William Scott Crawley.
Peter Ennis.
W. S. Furby.
Ernest Hand.
H. C. Harold.
James Jerritt.
Gisbert Kapp.
Henry Frederick William Lewis.
Peter John Nelson.
William Frederick O'Brien.
George Offor.
Robert Rigby.
Daniel Sinclair.
The Hon. Howard Spensley.
Frank Tremain.
Charles Wapshare.

The meeting then adjourned until Thursday evening, February 22nd, 1883.

THE LIBRARY.

The following letter has been addressed by the Librarian to the Local Honorary Secretaries, many of whom have promised assistance. Presents of books and pamphlets have also been received from Berlin, Rome, and Paris:—

Society of Telegraph Engineers and of Electricians,
4, The Sanctuary, S.W.,
London,
January, 1883.

Dear Sir,

In the number of the Journal of the Society which has been published, containing the Annual Report of the Council, will also be found a copy of my Annual Report, to which I beg to draw your attention, especially to that portion of it containing the List of Accessions to the Library during the past year. A perusal of this list will show you that I am making strenuous efforts to complete the collection under my charge, both as regards the modern works and those of the older ones which were not in the Library.

I am of opinion that the Library should contain copies of everything relating to our subject, not only books, but pamphlets, reports, manufacturers' catalogues, circulars, and in fact everything, however unimportant it may apparently be.

I am fairly able to obtain the greater part of the English publications and the most important foreign ones, but the foreign pamphlets I have great difficulty in securing.

I write therefore to ask for your kind co-operation and assistance in the object which I have in view, by forwarding me any works relating to our subject which may come to your notice. In any cases where you have to purchase the works I would remit your charges.

I may add, with regard to the most important books, that I usually receive notice of them from our booksellers, and I would not trouble you to inform me of these; my object is more particularly to obtain pamphlets and the less important publications relating to our subject.

I am, dear Sir,
Yours faithfully,
A. J. FROST,
Librarian.

The following is a copy of an autograph description of Reis's Telephone, which has been presented to the library by Mr. Wm. Ladd, Member.

[COPY.]

INSTITUT GARNIER,
FRIEDRICHSDORF.

DEAR SIR,

I am very sorry not to have been in Francfort when you were there at Mr. Albert's, by whom I have been informed that you have purchased one of my newly-invented instruments (telephons), though I will do all in my power to give you the most ample explanations on the subject. I am sure that personal communication would have been preferable, specially as I was told that you will show the apparatus at your next scientifical meeting, and thus introduce the apparatus in your country.

Tunes and sounds of any kind are only brought to our conception by the condensations and rarefactions of air or any other medium in which we may find ourselves. By every condensation the tympanum of our ear is pressed inwards, by every rarefaction it is pressed outward, and thus the tympanum performs oscillations like a pendulum. The smaller or greater number of the oscillations made in a second gives us, by help of the small bones in our ear and the auditory nerve, the idea of a higher or lower tune.

It was no hard labour, either to imagine that any other membrane beside that of our ear could be brought to make similar oscillations, if spanned in a proper manner and if taken in good proportions, or to make use of these oscillations for the interruption of a galvanic current. However, these were the principles which guided me in my invention: they were sufficient to induce me to try the reproduction of tunes at any distance. It would be long to relate all the fruitless attempts I made until I found out the proportions of the instrument and the necessary tension of the membrane. The apparatus you have bought is now what may be found most simple, and works without failing when arranged carefully in the following manner:—

The apparatus consists of two separated parts, one for the singing station, A, and the other for the hearing station, B.

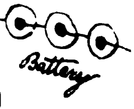
The apparatus A is a square box of wood, the cover of which shows the membrane, *b*, on the outside, under glass. In the middle of the latter is fixed a small platina plate to which a flattened copper wire is soldered, on purpose to conduct the galvanic current. Within the circle you will further remark two screws: one of them is terminated by a little pit in which you put a little drop of quicksilver, the other is pointed. The angle, which you will find lying on the membrane, is to be placed according to the letters, with the little hole *a* on the point *a*, the little platina foot *b* into the quicksilver screw, the other platina foot will then come on the platina plate in the middle of the membrane.

The galvanic current coming from the battery (which I compose generally of three or four good elements) is introduced at the conducting screw near *b*, wherefrom it proceeds to the quicksilver, the movable angle, the platina plate, and the complementary telegraph to the conducting screw, *s*. From here it goes through the conductor to the other station, B, and from there returns to the battery.

The apparatus B, a sonorous box on the cover of which is fixed the wire spiral with the steel axis, which will be magnetic when the current goes through the spiral. A second little box is fixed on the first one, and laid down on the steel axis to increase the intensity of the reproduced sounds. On the small side of the lower box you will find the corresponding part of mentary telegraph.

If a person sing at the station A, in the tube, x, th of air will pass into the box and move the memb thereby the platina foot c of the movable angle wi up, and thus will open the stream at every condensati the box. The stream will be re-established at every In this manner the steel axis at station B will be ma for every full vibration, and, as magnetism never enters a metal without disturbing the equilibrium of the steel axis at station B must repeat the vibrations at and then reproduce the sounds which caused them. will be reproduced if strong enough to set the me motion.

The little telegraph which you find on the sid apparatus is very useful and agreeable for to give signal both of the correspondents. At every opening of the st next following shutting, the station A will hear a li produced by the attraction of the steel spring. Ano clap will be heard at station B in the wire spiral. By m the claps and producing them in different measures, yo able, as well as I am, to get understood by your correspo



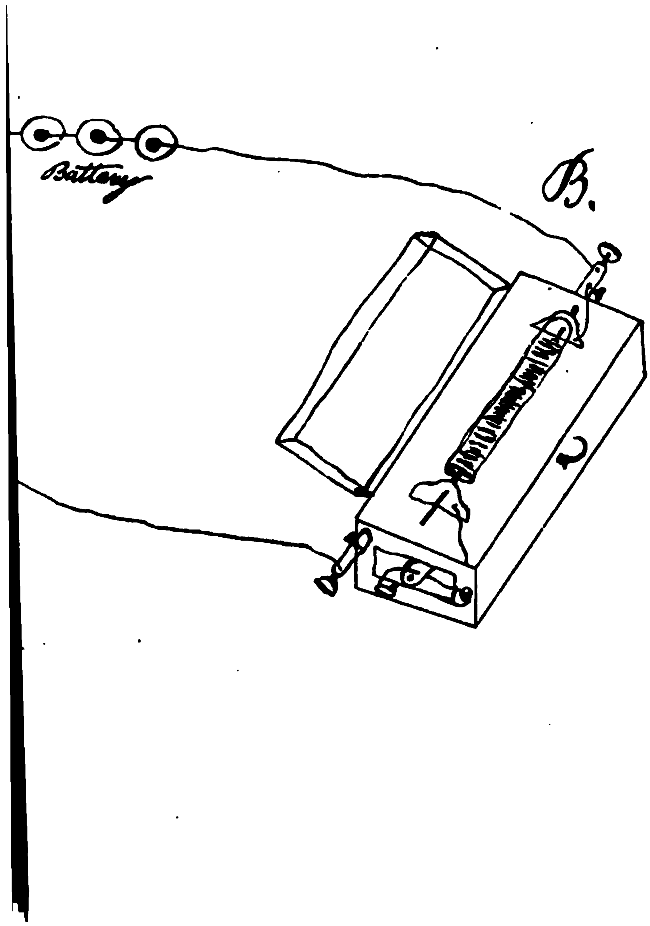
I am to end, Sir, and I hope that what I said will be to have a first try; afterwards you will get on quite alon

I am, Sir,
Your most obedient servan
PH.

FRIEDRICHSDORF, 18/7/63.

To Mr. William Ladd.

FAC SIMILE COPY OF REIS'S SKETCH.



ACCESSIONS TO THE LIBRARY DURING JANUARY, 1883.

By Alfred J. Frost, *Librarian.*

(*Works marked * have been purchased.*)

Parcels of books have been presented by Dr. Werner Siemens, Commandeur D'Amico, and the French Government.

Banneux [M. J.] La téléphonie a grande distance. 4to. 8 pp. [Ext. de "*L'Ingénieur-Conseil*" des 15, 31 Juillet et 15 Août, 1882.] *Bruxelles*, 1882

Bell [Prof. A. G.] [*Vide* Field, Kate.]

* **Bloch** [Jules]. Origines de l'électricité de la lumière de la chaleur et de la matière. La. 8vo. Première partie, 33 pp. Deuxième partie, 27 pp. *Nancy*, 1882

* **British Almanac and Companion, 1883.** Article on Electric Lighting, by L. T. Thorne, p. 82. 8vo. 287 pp. *London*, 1883

Chelsea. Report on Electric Lighting. [*See* Stayton, G.]

Conférence Internationale pour la détermination des Unités electriques. Resolutions passed at. Fo. 8 pp. *Paris*, 1882

——— 16 Octobre—26 Octobre, 1882. Proces-Verbaux. Fo. 163 pp. *Paris*, 1882

Clark [Latimer]. Transit Tables for 1883, giving the Greenwich mean time of the transit of the sun and of certain stars, for every day in the year, with an ephemeris of the sun, moon, and planets, and other information. 8vo. 103 pp. *London*, 1883

Dethridge. [*Vide* Poley & Dethridge.]

* **Du Moncel, Preece, Howell, and Siemens.** Incandescent Electric Lights, with particular reference to the Edison Lamps at the Paris Exhibition, by Comte Th. Du Moncel and Wm. Hy. Preece, to which is added "The Economy of the Electric Light by Incandescence," by John W. Howell, and on the "Steadiness of the Electric Current," by C. W. Siemens. 18mo. 176 pp. *New York*, 1882

* **Electro-Techniker, Der.** Organ für angewandte Electricität. 4to. Bd. I. *Wien*, 1882

* **Fachmann.** Die elektrotechnische Revolution. 8vo. 76 pp. *Munchen*, 1883

* **Field** [Kate], Editor. The History of Bell's Telephone. 8vo. 67 pp. *London*, 1878

* **Fitzgerald** [J. V. Vesey]. The Electric Lighting Act, 1882 (45 and 46 Vic., cap. 56), and the rules issued under the Act, with short explanatory notes and cases, and the several Acts incorporated therewith. 8vo. 123 pp. *London*, 1882

Galliani [Enea]. Metodo per calcolare la resistenza degli apparecchi e il numero delle coppie elettriche nei circuiti telegrafici. 8vo. 59 pp. *Ancona*, 1882

Giannini [Alamiro]. Guida amministrativa ad uso degli esercenti gli uffici telegrafici governativi di 3$^{a.}$ categoria. Demy 12mo. 227 pp. *Ravenna*, 1876

——— L'Amico del telegrafista. Memoriale-Agenda di Gabinetto per l'anno, 1878, compilato da Alamiro Giannini. 8vo. 144 pp. *Firenze*, 1878

Giannini [Alamiro.] Vade-mecum degli aiutanti delle sezioni telegrafiche dello stato. Sm. 12mo. 99 pp. *Roma*, 1882

* **Glen** [W. C.] and **Glen** [Alex.] The Electric Lighting Act, 1882. Sm. 8vo. 247 pp. *London*, 1882

Gravier [A.] Distribution of Electricity by canalisation for every kind of application. 4to. 23 pp. [Edited by W. Grist.] [*International Electric Exhibition, Crystal Palace*, 1882]. *London*, 1882

* **Grierson** [Thos. B.] Electric Lighting by Water Power. 8vo. 44 pp. [Received December 18th, 1882.] *Dublin*, 1883

Grist [W.] [*Vide* Gravier, A.]

Halske. [*Vide* Siemens and Halske.]

Howell [J. W.] [*See* Du Moncel and others.]

Iron and Steel Institute. Journal and List of Members, 1882. 8vo. 839 + lxii. pp. *London*, 1882

Italian Government. Guida degli Impiegati Telègrafici. I. Parte Tecnica. Costruzione e Manutenzione delle Linee Telegrafiche. 8vo. 164 pp. *Firenze*, 1877

——— Guida degli Impiegati Telegrafici. II. Parte Tecnica. Impianto, Manutenzione ed esercizio degli uffici. 8vo. 108 pp. *Roma and Firenze*, 1879

——— Relazione Statistica sui telegrafi del regno d'Italia nell' anno 1881. 4to. 197 pp. *Roma*, 1882

——— Istruzioni sul servizio dei circuiti a corrente continua. 8vo. 8 pp. *Rome*, 1883 [?]

Jamieson [A.] [*See* Rankine, W. J. M.]

Japing [Eduard]. Die elektrische Kraftübertragung und ihre Anwendung in der Praxis. Elektro-technische Bibliothek. Band II. 8vo. 239 pp. *Wien, Pest, Leipzig*, 1883

* **Kabath** [N. De]. A few practical remarks on the formation and use of N. de Kabath's patent Electric Accumulators. 8vo. 58 pp. *London*, 1883

Kew Observatory. Report of the Kew Committee for the year ending October 31, 1882. 8vo. 25 pp. [*Proc. Roy. Soc.*, 1882.] *London*, 1882

* **Mercantile Shipping Register and Commercial Review.** The Electric Light Companies. Full details of the constitution, progress, and resources of all the Companies, with explanatory tables and remarks. &c. 18 pp. Special Annual Supplement. December. *London*, 1882

Meyer. Descrizione dell' apparato multiplo. 8vo. 23 pp. Plates. *Firenze*, 1875

Obach [Dr. Eugen]. Eine Batterie für Starken Strom und langandauernde constanz. 8vo. 21 pp. [Carl's *Repertorium für Experimental-physik.* Bd. XVIII.] *München*, 1882

* **Poley** [Arthur P.] and **Dethridge** [Frank]. A Hand-book on the Electric Lighting Act, 1882, with hints to Local Authorities, Explanatory Notes, and Amended Rules of the Board of Trade. Sm. 8vo. 96 pp. *London*, 1882

Preece [W. H.] [*See* Du Moncel and others.]

Rankine [W. J. M.] Useful rules and tables relating to Mensuration, Engineering, Structures, and Machines. 6th Ed. Revised by W. J. Millar, with appendix, tables, tests, and formulæ for the use of Electrical Engineers, by A. Jamieson. 8vo. 384 pp. [Recd. Dec., 1882.] *London*, 1883

Santoni [G.] Il telegrafo automatico di Wheatstone. 8vo 99 pp. Plates. *Napoli*, 1882

* **Schoentjes** [H.] Les grandeurs électriques et leurs unités. La. 8vo. 107 pp. Plates. *Gand*, 1882

* **Schwartze** [Th.] Katechismus der Elektrotechnik. Ein Lehrbuch für praktiker, techniker und industrielle. 8vo. 294 pp. *Leipzig*, 1883

Secundärbahn-Zeitung. No. 45. II. Jahrgang, 8th November, 1882. Die electrische Grubenbahn in Zaukeroda [Sachsen]. 4to. Pages 452 to 454. *Berlin*, 1882

Siemens [C. W.] [*See* Du Moncel and others.]

Siemens and Halske. A. Wissenschaftliche und Mess-Instrumente. La. 8vo. 23 pp. *Berlin*, 1881

—— C. Telegraphen. La. 8vo. 46 pp. *Berlin*, 1881

—— D. Telegraphen-signal-und Sicherungs—Einrichtungen für Eisenbahnen. La. 8vo. 34 pp. *Berlin*, 1881

—— Reproduction de l'unité de résistance à mercure et relation du système des mesures électriques en usage dans l'établissement de Siemens et Halske à Berlin. 4to. 16 pp. *Berlin*, 1881

—— Gebrauchs—Anweisung zu den Torsionsgalvanometern mit Widerstandskasten, 4to. 4 pp. *Berlin*, 1882

—— [*See Secundärbahn-Zeitung.*]

Silver [S. W.] Catalogue of the York Gate Geographical and Colonial Library. La. 8vo. 184 pp. *London*, 1882

Smith [Willoughby]. Inaugural address to Society of Telegraph Engineers and of Electricians. La. 8vo 29 pp. *London*, 1883

Stayton [Geo. H.], C.E. Report on Electric Lighting. Vestry of Chelsea. Also the proceedings of the Vestry on the 5th December, 1882, thereon. 8vo. 32 pp. *London*, 1882

Thomson [Sir Wm.], D.C.L., F.R.S. Instructions for the adjustment of Sir William Thomson's patent compass of 1879. 8vo. 22 pp. *Glasgow*, 1880 [?]

Thorne [L. T.] [*Vide* British Almanac and Companion, 1883.]

* **Urquhart** [J. W.] Electro-motors: a treatise on the means and apparatus employed in the transmission of electrical energy and its conversion into motive power. Sm. 8vo. 178 pp. *Manchester*, 1882

Vicentini [G.] Gli elettromagneti. 8vo. 45 pp. *Roma*, 1882

* **Wilkins** [Bishop J.] Mathematical Magick; or, the Wonders that may be performed by Mechanical Geometry. 8vo. 295 pp. Portrait. *London*, 1680

Wheatstone. [*Vide* Santoni G.]

* **Zahn** [Dr. W. v.] Untersuchungen über contactelektricität. La. 8vo. 59 pp. Plate. *Leipzig*, 1882

Zetzsche [Prof. Dr. K. E.] Katechismus der elektrischen telegraphie. 8vo. 453 pp. [Received 1882.] *Leipzig*, 1883

ABSTRACTS.

MARCEL DEPREZ—LAW OF VARIATION OF E.M.F. OF A MAGNETO MACHINE WITH THE EXTERNAL RESISTANCE.

(*Comptes Rendus, Tome* 94, *No.* 24, *June* 12, 1882, *pp.* 1586-1589.)

Whilst prosecuting his researches on the "characteristic" of various dynamo machines, the author was struck by the fact that the E.M.F. produced in the ring of these machines, instead of increasing continually with the strength of the current circulating in the electro-magnets, ends by decreasing very considerably, whilst the current still continues to increase.

This observation led the author to the conclusion that the E.M.F. produced by a machine with permanent magnets is not a constant quantity for each speed, as has been generally assumed, but that it is a function of the current generated in the helix.

He put his theory to the test by experimenting with a Gramme electro-plating machine, the electro-magnets of which were separately excited, and the external resistance in circuit varied. From the predetermined "characteristic" of the machine, it was known that the electro-magnets were saturated when a current of 35 ampères circulated in their coils, and, in order that they might be as strongly magnetised as possible, a current of 80 ampères was passed through them. The speed of the machine was kept sensibly constant, varying in the first series of experiments from 2,431 to 2,420 revolutions per minute; the total resistance of the ring and external circuit was varied by small quantities at a time, from 3·712 ohms to 0·187 ohms, there being seventeen observations in all. The current, which varied from 8 ampères to 115·2 ampères, was measured on a Deprez galvanometer, and the E.M.F. was calculated by the formula $E = IR$. With a resistance varying from 3·712 ohms to 0·371 ohms, the current increased from 8 to 76·8 ampères, but the E.M.F. remained at about 29 or 28 volts; with 0·278 ohms, the current increased to an average of 90 ampères, while the E.M.F. dropped to 25 volts; with 0·187 ohms, the current was 115·2 ampères and the E.M.F. 21·54 volts.

In a second series of observations the speed was kept near 2,100 revolutions per minute, the greatest variation being 23 revolutions. The resistance in circuit was varied by twenty-five successive changes, and from 11·432 ohms to 0 653 ohms the E.M.F. was about 29 to 30 volts, while the current increased from 2·6 ampères to 43·2 ampères. The last seven observations are tabulated below, and show the marked falling-off in the E.M.F.:—

Speed.	Resistance. Ohms.	Current. Ampères.	E.M.F. Volts.
2,397	0·465	59·20	27·53
„	0·278	88·80	24·69
„	0·187	122·40	22·89
„	0·156	134·40	20·97
„	0·140	141·60	19·82
„	0·131	144·00	18·86
„	0·093	168·00	15·62

A third series of experiments was made with a dynamo-electric machine, the electro-magnets of which were separately excited by a current of 10·5 ampères, by which they were not nearly saturated. The values obtained show that the decrease of the E.M.F. is still more marked in this case where the magnetic field is not a powerful one. The last five observations may serve as example:—

Speed.	Resistance. Ohms.	Current. Ampères.	E.M.F. Volts.
2,805	0·595	40·8	24·28
„	0·434	48·6	21·09
„	0·288	58·8	16·93
„	0·132	76·8	10·14
„	0·050	84·0	4·20

The author considers that the phenomenon may be explained thus:—

If the current circulates in the helix alone, this will become a powerful magnet, and the line joining its two poles will coincide with the diameter passing through the points of contact of the brushes with the commutator.

If the current circulates in the electro-magnets alone, the line joining the poles of the helix will coincide with the line joining the poles of the electro-magnets.

When the current circulates, therefore, both in the helix and in the electro-magnets, the line joining the poles of the former will occupy an intermediate position, which may be determined by a construction similar to that of the parallelogram of forces. It is easy to see that the line joining the poles of the helix approaches more nearly to the line joining the brushes, the stronger the current is; whilst the maximum difference of potential between the two brushes corresponds to the case where the line joining the poles of the helix is perpendicular to the diameter drawn through the brushes.

This may be put into other words—*i.e.*, in proportion as the current circulating in the helix increases, the wires of the helix cut the lines of force of the magnetic field between the pole-pieces and the helix at an angle differing more and more from 90°, which is the angle corresponding to a maximum E.M.F.

The author is of opinion that this defect may in part be got over by employing (1st) very powerful electro-magnets, wound with a small quantity of wire, and (2nd) movable brush-holders.

JAMIN AND MANEUVRIER—THE CURRENT OF REACTION IN THE ELECTRIC ARC.

(*Comptes Rendus, Tome* 94, *No.* 25, *June* 19, 1882, *pp.* 1615-1619.)

The alternating currents produced by a self-exciting Gramme machine are exactly equal, and therefore do not decompose water, nor cause any deflection on a tangent galvanometer. This exact balance of effects still continues when one or more arc lamps are introduced into the circuit, provided always that the two carbons are equal in size, equally heated, and placed in similar positions.

On introducing eight or ten Bunsen cells into the circuit, when the machine is at rest, the tangent galvanometer will show a deflection δ, and when the machine is at work a deflection δ', equal to δ; thus in three experiments it was found that

$$\delta = 32,\ \delta' = 33;$$
$$\delta = 38,\ \delta' = 38{\cdot}45;$$
$$\delta = 33,\ \delta' = 34{\cdot}10.$$

It is thus proved that the resistance of the machine to an external current does not vary, whether the machine is working or not, and that the two effects of the machine and battery are independent and concurrent.

If, now, the current of the battery be interrupted and an arc lamp placed in the machine circuit, with unequal carbons, say, 4 mm. and 2 mm., a permanent deflection is produced on the galvanometer, showing that the two systems of alternating currents are no longer equal, but that one has the preponderance, viz., that one which goes from the larger carbon to the smaller, or, what is the same thing, from the cooler carbon to the hotter. The galvanometer shows a differential current, which attains its maximum when one carbon is a large block and the other a thin pencil. The same effect is produced when a carbon is used as one electrode of the lamp and a metal as the other, and in this case the strength of the differential current depends on the nature of the metal used. It was also observed that the current was small or nil for a short arc, but increased with the length of the latter.

The deflection depends upon, 1st, the mean E.M.F. of the differential current; 2nd, the resistance introduced into the circuit by the arc. Suppose that a battery of n elements is inserted in the circuit, with an E.M.F. $= nA$, accordingly as the current of the battery is in the same direction as the differential current or opposed to it, we shall have

$$I = \frac{e + nA}{R};\ I' = \frac{e - nA}{R}.$$

When e is greater than nA, the two deflections have the same sign, and

$$\frac{I}{I'} = K = \frac{e + nA}{e - nA} \therefore e = nA\frac{K+1}{K-1};$$

if e is less than nA, the deflections are in opposite directions, and

$$\frac{I}{-I} = K = \frac{e + nA}{nA - e} \therefore e = nA\frac{K-1}{K+1}.$$

In the actual experiments new Bunsen cells were used, and the following figures give the value in bunsens of the E.M.F. e of the differential current with different metals for one electrode:—lead, 2·5; iron, 3·2; carbon, 5; copper, 50·6; zinc, 66·2; mercury, 103·7.

The resistance can be found thus:

$$I - I' = \frac{2\,n\,A}{R} \therefore R = \frac{2\,n\,A}{I - I'}.$$

The total resistance, R, is made up of, 1st, the resistance of the machine; 2nd, the resistance of the battery; 3rd, the resistance of the arc; and as 1st and 2nd are constant, it is only the resistance of the arc which can vary; and from the above formula the less the value of I — I′, the greater the resistance.

The differential current can only be explained in two ways, either by a difference in the resistance of the arc, or by an inequality in the inverse reactions of the arc in one or the other direction.

To test if the resistance of the arc changed with the direction of the current, a continuous-current dynamo was employed to produce an arc between a carbon point and a surface of mercury; but on changing the direction of the current no difference was observed in the deflection of a galvanometer in a shunt circuit. It was, however, observed that, when the current passed from the mercury to the carbon, the arc was green, whilst in the inverse direction the arc was red; with an alternating Gramme machine, however, the arc was greenish, showing the preponderance of the system of currents passing from the mercury to the carbon over those in the opposite direction; and as there was no difference in the resistance of the arc, the cause of the differential current must be looked for in a property peculiar to alternating currents. This property may be explained. An original current (——>), at first very weak, gradually increases, and, when it ceases, causes an inverse reaction (<——), which is added to the current (<——) produced by the machine at the same moment. If, then, one of the systems of current (say, ——>) offers a weaker reaction than the opposed system (<——), it will be less weakened and more strongly reinforced, and will determine the direction of the differential current.

Whatever may be the explanation, it is anyhow certain that in the case of mercury one of the systems is considerably weakened, and that the other system consists of successive currents of greater intensity and duration. The action of an alternating machine with such an arc between a carbon and a mercury surface in the circuit will be completely changed, and it will act as a continuous-current dynamo, and can be used to decompose water, magnetise iron, deposit metals, or transport energy; and thus, by introducing one or more arcs between carbon points and a vessel of mercury, the commutator usually provided can be dispensed with.

G. LIPPMANN—THERMOSCOPIC METHOD OF DETERMINING THE OHM.

(*Comptes Rendus, Tome* 95, *No.* 15, *October* 9, 1882, *pp.* 634, 635.)

The present method differs from Dr. Joule's calorimetric method in not necessitating the measurement of any quantity of heat, nor the knowledge of the exact value of J.

The wire, the resistance, r, of which is to be determined, is placed in a vessel arranged as a calorimeter in an enclosure at a constant temperature. A current, the intensity, i, of which is measured, is passed through the wire, and is continued until, owing to the heat given off by the wire, the vessel has reached a stationary temperature; this point can be determined by a very delicate thermoscope placed in the vessel. The current is then interrupted, and a motor is set to work, which causes a rubbing inside the vessel containing the wire. The heat produced by the friction is thus substituted for that before given out by the wire. The friction is continued until the same stationary point of temperature is reached as before. If, then, W is the work expended in friction, we have $W = ri^2$, whence r can be determined. In practice it might be more convenient, first, to produce a stationary temperature by the friction, and then to reproduce it by changing the value of i. The author claims that his method has the advantage of not requiring the determination of the actual quantities of heat given out, as does Joule's method, since the work, W, and the electrical energy, ri^2, are both expended in the same vessel.

A. ERICH—IMPROVEMENTS IN THE CONDUCTION OF CURRENTS FOR ELECTRICAL RAILWAYS.

(*Zeitschrift für angew. Elektricitätslehre, B.* 4, *No.* 20, 1882, *pp.* 458-462.)

The system can be adapted either to ordinary tracks with raised rails, or to tramways having the rails level with the road. The conductor consists of an insulated wire running along by the side of the rail, and fastened by iron bands to the underside of a continuous wooden batten; the wire and batten are supported by iron brackets bolted to the side of the rail, preferably at the joints, so that the bracket may also serve as fish-plate, on one side at least. The insulated wire is cut up into sections of convenient length, say, 50 yards, and the ends, freed from the insulating material, project in the form of loops through holes bored for the purpose in the wooden batten, and are laid in grooves in its upper surface. Along the whole length of the batten are laid thin strips of steel, which are in electrical contact with the turned-up ends of the insulated wire, but separated from each other by a small space, which, however, is not so long but that one of the two contact-wheels attached to the carriage is always in contact with one length of steel strip, and the current is therefore never interrupted. The return is completed as usual through the left-hand rail.

For tramways much the same arrangement is adopted, but the insulated wire is laid in a channel cut in the top of the longitudinal wooden sleepers under the rail, with the successive lengths of which it is put in connection by similar means to those already described.

JOURNAL

OF THE

SOCIETY OF

Telegraph Engineers and of Electricians.

VOL. XII. 1883. No. 47.

The One Hundred and Nineteenth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday, February 22nd, 1883—Mr. WILLOUGHBY SMITH, President, in the Chair.

The minutes of the Ordinary General Meeting held on Thursday, February 8th, 1883, were read and confirmed, and the names of transfers and new candidates were announced and suspended.

Donations to the Society's Library were announced as having been received from the following gentlemen:—Lieut. Arthur H. Bagnold, R.E., Mr. J. Angelo Fahie, M. N. de Kabath, Dr. Donato Tommasi, Messrs. Friedrich Vieweg and Sohn.

A vote of thanks was accorded the donors.

The PRESIDENT: Owing to the long, and, I must say, interesting discussion which took place at our last meeting, the hour was too far advanced to allow Mr. A. J. S. Adams to reply. Therefore, before entering into the special business of this evening, I will ask him to favour us with any remarks he may have to make.

Mr. A. J. S. ADAMS: In referring to the sun's heat and light forces, I had no wish to dogmatise opinions of my own, but rather to suggest wherein, as appeared to me, lay the first causes for these electrical storms, and to call forth information from those who, by reason of advantageous position, rare opportunity, and the splendid means at their disposal, are well qualified to give it.

Little information was forthcoming as regards the sun, however, except an admission of the fact pointed out in my paper, that the position of solar spots upon the sun's face really does influence these electric storms.

From the general tone of the discussion, it seemed that my having introduced the sun's physical constitution into the question was considered to be in bad taste, but surely, to be logical, f these electric storms are referable to the solar orb, we must seek in the condition of the sun for their causation. This I sought to do, although the hypothesis suggested met with opposition.

It is doubtful if there is a scientist—notwithstanding the theory recently advanced by one of the great thinkers of the day (I refer to Dr. C. W. Siemens)—who would record an opinion that the sun's light and heat were produced by combustion, and yet the hypothesis I advanced renders combustion unnecessary. But the opposition experienced by what I advanced with regard to the sun's nature arose, probably, by reason of a widely different point of view it may be my misfortune to take, as against that of the gentlemen who took part in the discussion.

The effect of one heavenly body upon another is sought to be accounted for by calculations based upon the elements of mass, distance, and static charge, or electrical quantity, but it seemed to me that those elements failed to account for the observed effects, and that a further element was wanted. My mind has long pictured matter as being resolvable into ultimate units of like nature, each unit revolving about its axis, and that that motion constitutes what we term electricity; the polarisation consequent upon that motion being considered to be the effect to which we give the name of magnetism. My mind pictures the heavenly bodies as simply rotary, and hence polarised, accumulations or agglomerations of rotary units, by reason of which they became themselves endowed with the attributes of magnetic and inducive forces; and here is the point of variance. Many are no better able to understand than myself how the fact of simple static charge can account for these electric disturbances;

but I bring into the question the force of polarisation in addition to mere mass, distance, and charge, and thus, in referring to the sun's atmosphere as cutting lines of force thrown out by the nucleus, it was to the lines of force due to polarisation rather than to simple induction due to quantative or static charge.

Professor Adams took exception to this hypothesis, in that any electrical condition of the nucleus would be imparted to the sun's atmosphere, and that we should thus, after all, have to deal with the sun's atmosphere as the prime factor. But I failed to learn whether such impartation was by conduction or induction. If the former, by what means is conduction between the sun's nucleus and atmosphere effected? If by induction, then I claim the point in my favour. A misapprehension seems to have arisen, by which my paper was misunderstood to say, that the sun's nucleus was only *visible* through the opening some six days after the first appearance of the spot upon the eastern limb. My paper, I trust, says nothing of the kind; indeed, I am not aware that the nucleus is visible under any possible circumstances. My suggestion was, that it appeared from observation probable that only when the exposed portion—electrically, not visually exposed—of the nucleus was in a line with the earth through the opening in his atmosphere that these storms were felt.

Another difficulty in the way of my hypothesis was stated to be the fact that less heat was received from the sun during the presence of solar spots than at other times, and that hence we should look to thermal causation for electric storms. But, if I mistake not, the loss of heat due to solar spots is so slight as to amount only to a matter of fact, and to be quite inadequate to the production of these disturbances, whilst the fact that a variation in the sun's heat will affect the *earth as a whole* is not calculated to produce thermal-electro disturbances. I, however, lay claim to the loss of heat during a sun-spot as corollary evidence in my favour, insomuch that the lines of force thrown into space through the opening in the sun's atmosphere must be lost to that atmosphere as light and heat producers.

I am obliged to Professor Perry for his observations in reference to my diagram of the sun; I would, however, point out that the diagram, if proportioned, would have defeated its object, which was to illustrate an idea, and not to give a lesson in astronomy.

Much was sought to be made of physical geography in the discussion, and rightly, when we remember that the earth, although a container, is not necessarily a conductor of electricity, and that the earth's moisture plays a part in relation to the effects under discussion; still, physical geography and physical effects are one thing—physical geography and first cause are quite another. I have to thank the members for the kindness with which my paper was received, and for the remarks made upon it.

The following paper was then read:—

ON THE MEASUREMENT OF ELECTRICITY FOR COMMERCIAL PURPOSES.

By James N. Shoolbred, B.A.,
Mem. Inst. C.E. and Soc. Tel. Engrs.

The present remarks are intended to deal more especially with the commercial question, raised by what is termed the "supply of electricity," in the Electric Lighting Act, 1882; the objects of such supply being more particularly lighting for public and private purposes, and the transmission of power. All these require electric currents of such magnitude as are generally produced by dynamo machines. To such branches of the subject as telegraphy and telephony, excepting in the general principles common to electric currents, the scope of the paper, and especially the special form of the various apparatus described, will but indirectly apply. The remarks must, in fact, be taken as mainly referring to an electric supply by mechanically-generated induction currents.

The basis and principle upon which rests this mode of producing electricity is the conversion of mechanical into electrical energy. Any exact financial evaluation of the commodity produced and offered for sale involves a knowledge, by measurement, of the amount of this energy; not merely of the electrical energy as offered to the customer, but also of the mechanical, which is expended and absorbed in such offer. The former is a question mainly for the consumer, the latter for the producer; who must, however, check the two together to enable him to arrive at the fair marketable value of the supply which is offered for sale.

In most cases the evaluation of the mechanical energy expended resolves itself into coal, wages, engine-room expenses, outside working and collection charges, interest on capital expended, and depreciation on plant employed; but chiefly on the two first named items—coal and wages.

It is not intended in these remarks to deal with this portion of the subject, which is one of statistics; but with the more pertinent one, to this Society at least, of how to measure and value the electrical product resulting from the above expenditure. Before dismissing the former, the mechanical branch of the subject, it may be well to advert to the similarity, in most cases, and taken broadly, in the nature of the conditions of manufacture and of supply of electricity and of gas, both for illumination and for power.

Each starts with coal, the energy of which it transforms into a commodity for the purposes just named; and it is in the process of transformation, in the nature and costliness of the intermediate operations, and in the amount and value of the plant and working expenses involved in each system respectively, that the main commercial difference between the two must exist. Iron forms a large part of the material of the plant necessary in both, and coal and wages, subject to the same conditions and variations, form the main bulk of the working expenses of the two systems.

This similarity in objects and in operations is here referred to, in order to point out that, as it is by the careful experience of more than half a century the conditions, both legal and social, have been ascertained for the supply of gas (and also, it should be added, of water), it need not, therefore, be surprising if some of these conditions, stamped with the seal of every-day practice, should be found to adapt themselves, or to be worth while taking into consideration, in the case of electricity; the differences in the processes of production, and of the products themselves, being always remembered.

The evaluation of the electrical energy of the supply depends upon the exact measurement of two factors—the amount of the supply and the pressure under which it is given; or, in other words, the quantity of the current, and the difference of potential (or electro-motive force) which the consumer is able to avail himself of.

As there appears to exist a misconception, in some quarters, as to the exact terms in which the various electrical units have been defined, and the precise duty of each, more especially as to the "ampère" and the "coulomb" (for each of which the term "weber" was formerly and almost indiscriminately used), it may be well to give the sense of the resolutions passed at the International Congress of Electricians, held in Paris in 1881, and now universally adopted.

ELECTRICAL UNITS.

The fundamental units adopted are those of the centimètre (length), the gramme (mass), and the second (time); or, as it is called for brevity, the C.G.S. system.

For practical units the following are adopted:—

The *Ohm* (10^9 C.G.S. units), as the unit of resistance, is that of a column of mercury having one square millimètre of section, and of a length hereafter to be determined by a commission specially appointed for the purpose.

Note.—The length is supposed, however, to be between 104 and 105 centimètres.

The *Volt* (10^8 C.G.S. units of electro-motive force), as the unit of electro-motive force.

Note.—This corresponds nearly to that of a Daniell's cell.

The *Ampère* (10^{-1} C.G.S. units of current), as the unit of current; which is the current produced by one volt through one ohm.

The *Coulomb* (10^{-1} C.G.S. units of quantity), as the unit of quantity of electricity; which is defined by the condition that an ampère yields one coulomb per second.

The *Farad* (10^{-9} C.G.S. units of capacity), as the unit of capacity; which is such that one volt in a farad shall give one coulomb.

Or, to quote the words of Sir Wm. Thomson (the chairman of the British Association Committee which settled the C.G.S. system of units above referred to), when explaining the practical units to the Congress:—"The volt acting through an ohm gives a current of one ampère, that is to say, one coulomb per second; and the farad is the capacity of a condenser, which holds one coulomb, when the difference of potential of its two plates is one volt."

The following were also suggested by Dr. C. W. Siemens, at the British Association meeting, 1882, to be added to the above units:—

The *Watt* (10^7 C.G.S. units of power), as the unit of power; being the rate at which work is done by a current of one ampère flowing in a conductor whose ends differ in potential by one volt.

The *Joule* (10^7 C.G.S. units of work), as the unit of work, or heat, being the heat generated by a watt in a second.

Although the unit of quantity, as defined, is the coulomb, yet it is sometimes found more convenient to express measurements of quantity in terms of the current and the time.

Thus, it is more convenient, sometimes, to say "one ampère-hour" than "3,600 coulombs," as the quantity of electricity conveyed by a current of one ampère flowing continually, and uniformly, for the space of one hour.

The product of the quantity of electricity conveyed, in ampère seconds, or in coulombs, by the electrical pressure of the same, expressed in volts, gives the electrical energy expended, in joules.

The electrical energy is therefore represented by the product of volts × ampères × time; or, by the product of volts × coulombs; or, expressed algebraically, $W = E\,C\,t = E\,Q$.

The following are the equivalent expressions for the same amount of power, only expressed in other terms, some of which may perhaps be more familiar:—

Rate of expending Energy.

$$1 \text{ volt-ampère} = \begin{cases} 10^7 \text{ ergs per second.} \\ 1 \text{ watt.} \\ \frac{1}{1\cdot 35} \text{ foot-pounds per second.} \\ \frac{1}{9\cdot 81} \text{ kilogrammètres per second.} \\ \frac{1}{736} \text{ force cheval (French horse-power).} \\ \frac{1}{746} \text{ horse-power.} \end{cases}$$

It is suggested in some of the (draft) provisional orders for the supply of electricity, now before the Board of Trade, that the unit of price to be charged should be based on "the energy contained in a current of 1,000 ampères flowing under an electro-motive force of one volt during one hour"; or, in other words, the unit might be put as 1,000 volt-ampère-hours. Since the ampère-hour is another way of saying 3,600 coulombs of quantity of electricity supplied, the above expression may be put thus, as representing the value of the above unit.

Work done, or Energy expended.

$$1{,}000 \text{ volt-ampère-hours} = \begin{cases} 10^{10} \text{ erg hours.} \\ 3{,}600{,}000 \text{ volt-coulombs.} \\ 3{,}600{,}000 \text{ joules.} \\ 1{,}000 \text{ watts hours.} \\ 2{,}645{,}000 \text{ foot-pounds.} \\ 366{,}840 \text{ kilogrammètres.} \\ 1\cdot 35 \text{ force-cheval-heures.} \\ 1\cdot 34 \text{ horse-power hours.} \end{cases}$$

Put in terms more in accordance with actual practice, the above unit might mean the supply for one hour of a current of 10 ampères with an electro-motive force of 100 volts.

To arrive at a due evaluation of the supply of electric energy, it is evident that the measurement of each of these two factors (in volts and ampères respectively), should be effected either separately or combined; and also that a continuous and cumulative record should be kept of the supply as it proceeds.

Many instruments exist for the measurement of the above elements, at any particular time; but without any means of continuously recording such measurements. These evidently, except under certain conditions, cannot comply with the commercial conditions required for ascertaining the amount of supply. It is only, therefore, instruments furnished with means of continuously integrating, or recording the successive progressions of the supply, or instruments to which such recording apparatus can readily be attached, that come properly within the scope of this paper.

To measure with completeness for commercial purposes, a supply of electricity will entail, therefore, a continuous record of each of the two elements just referred to, current and pressure, either separately or combined. In the supply of towns, however, the question for the consumer may, and will, most probably, be much simplified, by causing one of these elements, that of pressure, to remain constant; since it is very likely that a constant standard pressure of supply will be fixed by the Government in granting the several provisional orders. If so, it then becomes the duty of the suppliers to keep up to that pressure under penalty; and instruments for recording such pressure will have to be installed where required, and placed under proper supervision.

For the customer, however, it will then generally suffice to have an exact record of the quantity only of his individual consumption of electric supply.

It has been thought advisable to precede the description of the recording or registering instruments, which alone are "meters" in the commonly accepted sense of the word, by an enumeration of some of those non-recording instruments which are in more general use; since they suffice for present exigencies. This short descriptive enumeration is even almost necessary, since most of these instruments, by the addition of some recording apparatus, or appliance by which the element of time can be integrated, may be made to enter into the class of registering meters. Indeed, some of them already possess their representative in this second class, or else have given rise to some modification, which has complied with the requirements in the latter case.

Thus, any current or ampère measurer may be converted into a record of quantity, or a coulomb-meter, by the integration of the time during which the current has flowed; and, similarly, any power or volt-ampère measurer may become a register of work done by means of the addition of the elements of time. Again, volt or pressure measurers will always be required in any case where a check is required upon the actual difference of potentials, or electro-motive force of the supply; and this may arise from a variety of causes.

The following is a short description of some of the instruments in use for the purposes of measurement of electricity, a reference being given where, if desired, a fuller description may be found; and the illustrations mainly refer to those instruments which are, so far as the writer knows, not described elsewhere.

NON-REGISTERING INSTRUMENTS.

CURRENT MEASURERS.

*Ayrton and Perry's "Am-meter."**—This instrument is made in several forms. In its original form it consists of a permanent magnet of horse-shoe form, with a needle-armature fixed between its pole-pieces. These are made of such a shape as to ensure that the deflections of the needle shall be directly proportional to the intensity of the current causing them; hence a knowledge only of the constant of each instrument is required when using it. Furthermore, the coil of wire, through which the current passes, is in some instruments divided up into ten strands, which may by means of a cylindrical commutator be coupled up in "parallel" or in "series"; as it is desired, or not, to magnify the deflection tenfold for small currents. In cases where loss of magnetism in the permanent magnet is to be feared, a spiral spring is made to take the place of the horse-shoe magnet as the controlling force.

Again, where extreme delicacy is required, the deflections of the needle may be magnified tenfold or more, by attaching to the shaft of the needle a wheel and pinion. This form of instrument is adapted for use with strong currents.

Siemens' Electro-dynamometer† consists of a fixed coil, and of a movable coil suspended by a thread and a spiral spring, the normal position of the latter being at right angles to that of the former. When a current is sent through the two coils in series, the movable coil is deflected; this deflection is counteracted by torsion of the spiral spring, the amount of such torsion (indicated by a pointer on an index-dial) being proportional to the square of the current strength.

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* See *Journal Soc. Tel. Engrs.*, No. 36, Vol. X., April, 1881, and No. 43, Vol. XI., Sept., 1882.

† See *Journal Soc. Tel. Engrs.*, No. 43, Vol. XI., Sept., 1882.

Oback's Tangent Galvanometer[*] has a movable ring round a horizontal axis. If the ring is vertical the needle tends to turn on a vertical axis, as in the ordinary tangent galvanometer (the opposing force being the horizontal component of the earth's magnetism); if, however, the ring is placed horizontally, the needle turns round a horizontal axis, or dips (the opposing force being the vertical component of the earth's magnetism). The effect of the intensity of the current upon the needle may therefore be varied with the inclination of the ring. With the ring in any fixed position, the current strength is proportional to the tangent of the deflection (as in an ordinary tangent galvanometer).

Cardew's Low-resistance Galvanometer[†] is based on the principle of finding the intensity of an unknown current by balancing it against one of known strength, and thus ascertaining the relation between the two. It consists of two coils, a thick wire one (for the unknown current), and a thin wire one (for the standard one): both are wound on the same bobbin, but are insulated from each other. A magnet, with needle-pointer, is pivoted in the centre of the bobbin, and its deflections are counteracted by the insertion of known resistances.

Deprez's Galvanometer consists of a horse-shoe permanent magnet, with a solid armature-needle and index-pointer, pivoted vertically, placed between its poles, and a coil of the wire, which carries the current to be measured, laid within the horse-shoe. A table indicating the value of the several deflections of the needle has to be used.

Sir Wm. Thomson's Current Galvanometer.[‡]—This instrument, together with its fellow one, for the measurement of the difference of potential, have been termed "graded galvanometers." They are designed so as to permit of a very wide range being obtained with each respectively; this being effected by varying the intensity of the magnetic field in a known ratio, and comparing with it the unknown current.

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[*] See *Journal Soc. Tel. Engrs.*, No. 43, Vol. XI., Sept. 1882.

[†] Ibid.

[‡] See *Nature*, Sept. 21, 1882.

The present instrument consists essentially of two parts—1st, a coil of thick wire, through which the current to be measured is allowed to pass, it being fixed upright at the end of a wooden platform, with about one-half only of the coil projecting above it; and, 2nd, of a "magnetometer." This last consists of a system of four short steel magnets (constituting the "needle") mounted in a frame on a vertical axis, with a long aluminium pointer attached, and enclosed in a quadrant-shaped box, with a glass cover, and a silvered glass mirror bottom (in order to counteract any effect of parallax while reading the scale of tangents, to which the instrument is graduated).

In order to increase the directive force of the needle, when required, a semicircular permanent magnet of hard steel and of known intensity may be fixed upright on two arms projecting from the magnetometer behind the apex of the quadrant. The magnet stands over the needle, with its magnetic axis in the horizontal plane through that of the needle.

The magnetometer is made to slide along a groove in the wooden platform in front of the fixed coil, and in direction at right angles to it, the axis of the magnet and also of the needle (when at rest) being parallel to that of the coil; while the aluminium pointer, when at zero, which is the centre of the scale, lies in direction with the groove, or at right angles to the coil.

The range of sensibility obtained by varying the extreme distance along the groove of the magnetometer from the coil is about fifty-fold; while by removing the magnet, and leaving the needle under the influence of the earth's force alone, a further sensibility of fifty times greater can be obtained.

PRESSURE MEASURERS.

Ayrton and Perry's "*Volt-meter*" is in principle and form like their am-meter, and it has, like it, several similar modifications. The essential difference between the two instruments is that the thick wire coil of the am-meter is here replaced by a thin wire coil, with the connections to its ends so arranged as to measure the difference of potential between the points desired.

Sir William Thomson's "Potential" Galvanometer differs only from his "current" instrument, already described, in the use of a thin wire coil (of German silver of about 6,000 ohms resistance), instead of a thick wire coil, and in the change in the connections to suit the present object.

Deprez's Galvanometer.—Here, again, this instrument bears the same relation to his current measurer, and differs from it in a way similar to that in the preceding cases.

*Siemens' Torsion Galvanometer** consists of a bell-magnet, suspended between two coils of fine wire by means of a thread and a spiral spring. The deflections of the magnet under the influence of the current are counteracted by means of the spring, as in their electro-dynamometer, only that the angles of torsion are here proportional simply to the intensity. Certain resistances can also be added by which the sensitiveness can, by means of the insertion of a plug, be increased tenfold.

POWER, OR ENERGY MEASURERS.

*Ayrton and Perry's "Power-meter"** consists of a thick wire coil through which the main current passes, and of a fine wire coil on a shunt, which is suspended within it. The axis of the two coils are parallel, and the passage of a current tends to deflect the suspended one. Since the thick wire coil measures the intensity of the current, and the thin wire one its electro-motive force, and as the deflection of the latter is the product of the two, therefore the amount of that deflection is the measure of the power, or energy, of the current, and being directly proportional to the latter, can be ascertained without the use of a table.

Modifications of this instrument, with or without a commutator, or with a wheel and pinion arrangement, are also made.

Siemens' Watt-meter.—This instrument is constructed on the same principle as the electro-dynamometer. The two coils are, however, kept distinct from one another, with separate terminals to each. The fixed one, of thin wire, on a shunt, measures the electro-motive force; while the movable one has the main current passing through it. The resulting position of the latter coil is due directly to the product of the intensities of the two currents; that is, to the power developed. This product, of volts × ampères, is watts; hence the name of the instrument. The angle of torsion of the index-pointer gives the watts directly, and without the intervention of a table.

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* See *Journal Soc. Tel. Engrs.*, No. 48, Vol. XI., Sept., 1882.

RESISTANCE MEASURERS.

Although measurements of this class are generally carried out by means of a galvanometer, a set of resistance coils, and a Wheatstone bridge (therewith balancing the unknown resistance by means of one of a known amount), yet amongst the instruments exhibited is the following one. It is intimately connected with some of those already described, and its internal arrangements are based upon one of the fundamental laws of electricity, that of Ohm; which states that resistance $= \frac{\text{E.M.F.}}{\text{current.}}$

Ayrton and Perry's "Ohm-meter"[*] consists of two coils: a thick wire one for the main current, and a thin wire one on a shunt for the electro-motive force. Both are fixed with their axes at right angles to each other. A needle is acted upon by the currents flowing through each, and as these are at right angles to each other, the resulting deflection represents the ratio $\frac{\text{E.M.F.}}{\text{current}}$ *i.e.*, resistance; while in the power-meter the deflection is caused by the product of current × E.M.F.

In all these different instruments devised by Ayrton and Perry, the deflections are caused by duly proportioning the parts, to be directly proportional to the measurements sought for in each case.

REGISTERING INSTRUMENTS.

These may be divided into two classes—1st, Quantity, or Coulomb-meters; and, 2nd, Energy, or Work-meters.

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[*] See *Journal Soc. Tel. Engrs.*, No. 43, Vol. XI., Sept., 1882.

1. *Quantity, or Coulomb-meters.*

These, again, are separated into those based upon, *A*, *electrolytic* action, and those which are, *B*, *mechanical* in principle.

A. Electrolytic.

EDISON'S CURRENT-METERS.

These are based upon electro-deposition of metal, due to the action of a known fractional part of the total current. The weight of the increments is ascertained periodically, and from it the total quantity of the current which has passed during the interval is deduced.

The metal now generally used consists of plates of amalgamated zinc, immersed in a solution of 90 parts of sulphate of zinc and 100 parts of pure water.

In the form of meter for commercial use, two cells are placed as a check against one another; one, termed the "monthly cell," receiving four times the current of the other, which is known as the "quarterly cell."

In order to prevent the temperature of the liquid in the cells falling so low as to freeze, a connection is made by means of a long thin strip of brass and steel riveted together to an incandescent lamp, which is thereby lighted and raises the temperature as required. It is only when the temperature falls to 42° Fah. that this tongue is sufficiently depressed to form contact, and so to light the lamp. On the temperature rising the tongue rises, and the lamp is extinguished.

Experience shows that electro-deposition, to give a true and reliable record, should not be forced or overworked in its action; and that the plates should not in their daily duty be required to do more work, or be longer in action than they are intended for by their superficial area. In practice, about 75 per cent. only of their nominal work should be required of them. It appears to be the practice to design the duty of a meter for three hours, burning each lamp per night. Thus, a 25-light meter would have its plates designed for 75 lamp-hours per night. As just stated, it is not advisable, in practice, to exceed 75 per cent. of this amount. However, it is seldom found that the whole of the lights are used for the entire of the time allotted to each; and, if this were so, it would merely require a larger meter to meet the case.

At the present moment, it is understood that over three hundred meters on this principle are in use in New York.

Several other forms of meter have been devised by Edison, such as his beam-meter, where, when the increments by electro-deposition have accumulated to a certain limit, the current is reversed and the accumulation is redissolved, to recommence again when the normal condition is gained; and, again, electric-motor-meters, by fluid pressure, etc. All of these have, it is said, in practice been found—in cases where the total current supplied is very small, and this often so—to require so large a proportion of it for these mechanical operations, as to make the record unreliable. The meter first described, by electrolytic action only, is therefore now generally adopted by Edison.

SPRAGUE'S METERS.

These instruments are based upon electro-deposit up to a certain point; *i.e.*, when the intended quantity of metal, whether copper or zinc, has been deposited on the plate. The current is then reversed, and the metal gradually dissolved again until the primary condition of the plate is reached; when, by another reversal of the current, deposition again commences. Each reversal of the current is recorded by a mechanical counter and a train and wheels.

Not much practical experience has so far been obtained with these meters; but what has been done tends to point out that the mechanical operations involved in the reversals of the current, and in their registration, absorb a large amount of power.

B. Mechanical.

HOPKINSON'S CURRENT-METER.

This instrument consists of a thick wire coil, in the form of a solenoid, through which the current passes to be measured. The iron core of this solenoid revolves with its central shaft by the action of the armature of a small dynamo machine, placed at one end of the shaft.

The core of the solenoid is in two parts; the lower is fixed to the shaft, while the upper is movable, being attached to a governor-ball arrangement, and sliding up and down the shaft in accordance with the variations in the rotation speed of the shaft. A shunt current passes through the dynamo and its armature, then up through the lower or fixed portion of the core, and (by contact only) to the sliding part, and thence to the framework of the apparatus.

If the movable core be lifted, owing to the speed of rotation by the action of the governor-balls, this circuit is broken, and the shunt current through the dynamo interrupted. Whenever a current to be measured passes through the coil, attraction, by means of its casing, takes place between the fixed and the movable parts of the iron core.

This magnetic action, which is proportional to the square of the current, tends to keep the two parts of the core together and in contact; while the centrifugal force of the governor-balls, which is proportional to the square of the speed of revolution, tends to break the contact by lifting the movable part. These opposite forces will, in working, balance one another, and the result is that the system revolves with a velocity proportional to the current through the coil.

As the revolutions of the shaft are transmitted continuously by a train of wheels to a set of index dials, a record is thus kept of the quantity of the current that has passed.

In the construction of the apparatus the weight of the core is taken off by springs. The arrangement of the parts, as now made, is shown in the sheet of figures.

BOYS' QUANTITY, OR VIBRATING METER.

This instrument is based upon two well-known principles. 1. The force acting on the armature of an electro magnet, in any position, is proportional to the square of the current. 2. The square of the number of vibrations, say, of a pendulum, is a measure of the controlling force.

Therefore, if the controlling force under which a body vibrates is due to the action of an electro-magnet on its armature, the square of the number of vibrations in a given time is a measure of the square of the electric current. In other words, the rate of vibrating is a measure of the strength of the current, and the number of vibrations is a measure of its quantity.

The exact form and nature of the meter may vary in many details. The one now shown in the figure consists, primarily, of an electro-magnet (the upper one), through the coils of which passes a portion of the main current to be measured. This magnet is placed horizontally, and a vertical rocking shaft stands between its poles. This shaft has fixed on it a soft iron armature, rounded at the ends, and free to move in the horizontal plane between the poles of the electro-magnet.

The intensity of the attraction between the poles and this armature determines the rate of vibration; which, as above stated, is a measure of the strength of the current. Each vibration is itself recorded by means of an escapement, a train of wheels, and a set of index dials; and the number of vibrations thus registered becomes a measure of the quantity of the current. To add to the momentum of the vibrating body, two long arms, weighted at the end, are attached to the lower part of the vertical shaft.

In order, likewise, to prevent the vibrating armature from gradually coming to rest, it is arranged that, when the vibrations fall below a certain limit, by making contact, a portion of the current is sent round the coils of a second or "impulse" electro-magnet (placed underneath the "controlling" magnet), and which has an armature of a suitable form fixed on to the same shaft that carries the armature of the upper magnet. The extra motion thus given to the shaft, by the attraction of the lower armature, affords the necessary impulse to the vibrating armature when required to do so.

Ayrton and Perry's Coulomb-meter * consists of a small magneto-electro-motor, on a shunt circuit, which is acted upon by fluid friction. As this fluid friction represents an opposing force directly proportional to the velocity, while the force causing motion is proportional to the current, hence the last named is proportional to the velocity. The revolutions of the motor being registered by a speed-counter, a record of the total quantity of the current which has passed is thus kept.

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* See *Journal Soc. Tel. Engrs.*, No. 43, Vol. XI., Sept., 1882.

2. *Energy, or Work-meters.*

BOYS' ENERGY-METER.*

This instrument consists of two parts—1st, the indicator of energy, and, 2nd, the integrating apparatus.

1st. In the indicator of energy, a balanced beam has from one end suspended a counterweight, and from the other a hollow solenoid, free to work up and down into two other fixed solenoids. The movable solenoid is wound with a considerable length of fine wire; in the upper half in one direction, in the lower in the opposite (this is to render it independent of any magnet which may be placed near to it). This solenoid constitutes the high resistance shunt which measures the E.M.F. The two fixed solenoids are wound with thick wire, and convey the main current.

The result of the action of the fixed and the movable solenoids on each other is a force proportional to the product of the two currents, that is, to the energy being expended; but the external evidence of this is the inclination of the beam, and this inclination, or rather the tangent of the inclination, is proportional to the energy being expended.

2nd. The recording apparatus consists of a cylinder, which by means of a mangle-motion is made to reciprocate backwards and forwards by clockwork, and during its passage in each direction the cylinder is made to bear alternately against one of two tangent wheels, each free to be inclined in its direction of travel: both are fixed on the same swivelling frame, but only one of them bears at the same time against the cylinder.

This frame is free to be inclined from the vertical in correspondence with the inclinations of the beam above mentioned;

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* See *Philosophical Magazine*, February, 1882.

but the tangent of the inclination of the beam, as has been said, is proportional to the energy of the current; so also, therefore, is the tangent of the inclination of the wheels $\left(i.e., \frac{d\,y}{d\,x}\right)$.

The effect of this inclination of the tangent wheel is to cause the reciprocating cylinder to rotate, the speed of such rotation being proportional to the tangent of the inclination of the wheel, which is likewise proportional to the tangent of the inclination of the beam; that is, to the amount of energy expended. The path of the tangent wheel on the reciprocating cylinder, when not inclined, is simply a straight line lengthways along the cylinder, and no rotation is caused; but when, owing to the inclination of the wheel, the cylinder rotates, the wheel-path becomes a spiral.

The rotations of the cylinder are transmitted to a train of wheels and registered; thus giving a record of the amount of energy expended during a given time.

BOYS' ENGINE-POWER-METER.

This instrument (shown in the figure) is based upon the principle of integration just described. It consists of an integrating cylinder carried in a frame which moves to and fro with the piston of the engine; being pulled one way by a wire, and the other by a spring. The cylinder is free to rotate upon its axle, which carries an arrangement for transmitting these rotations, by means of a train of wheels, to a set of index dials, and so recording them.

A tangent wheel in a fixed frame, the normal position of which is parallel to the axis of the cylinder, presses constantly against the side of the cylinder; it is, however, capable of pivoting in its bearings, and becoming inclined to the axis of the cylinder. A rod in the plane of the tangent wheel, and perpendicular to its pivoting shaft (to which it is fixed), slides through a collar attached on a pivot at the end of a rod, which carries a piston with a spring upon it; the upper and lower surfaces of this piston being in communication with the corresponding ends of the engine cylinder.

The variations in pressure upon the engine piston (whether from steam, water, &c.) are by this means communicated to the tangent wheel, causing its inclination to vary, and at the same time imparting an amount of rotation to the cylinder proportional to those variations in pressure. At the same time the reciprocations of the integrating cylinder are proportional to the speed of the engine piston. Therefore the amount of the rotations of the cylinder, all of which remains continuously recorded on the index dials, being proportional to the product of the pressure on the engine piston into the distance through which it travels, are a measure of the total power expended; that is, of the work done by the engine.

DEPREZ'S ENERGY-METER.*

This instrument consists of a thick wire coil, movable upon its axis, which passes through its centre of gravity. This axis-shaft is set upon two knife-edges, insulated from each other, and communicating respectively with the coil; and with a metal bar having a curved, quadrant-shaped head, which dips into a mercury cup, and thus forms connection with the main current. The shaft also carries, projecting downwards, a pendulum rod, the bob of which is intended to cause an antagonistic force; while the extension, upwards, of the rod forms a pointer, with a scale fixed on the frame of the apparatus. On the frame there is also a fine wire coil, on a shunt, fixed so as to surround the movable coil.

The deflections on the pointer are therefore due to the product of the intensity of the current and to its electro-motive force; that is, to the power developed.

The readings of this instrument may be integrated, either by a Deprez rotating disc and ball integrator; or by one designed by Abdank (of Cracow), having a travelling cylinder and tangent-wheel arrangement, somewhat similar to that of Boys.

AYRTON AND PERRY'S ERG-METER.

This instrument is but a further development or sequel to their power-meter; by the addition of apparatus which integrate and record continuously the time during which the electrical energy has been imparted, as well as the variations in its amount. By this means is preserved a record of the entire work done, or of the total electrical energy supplied.

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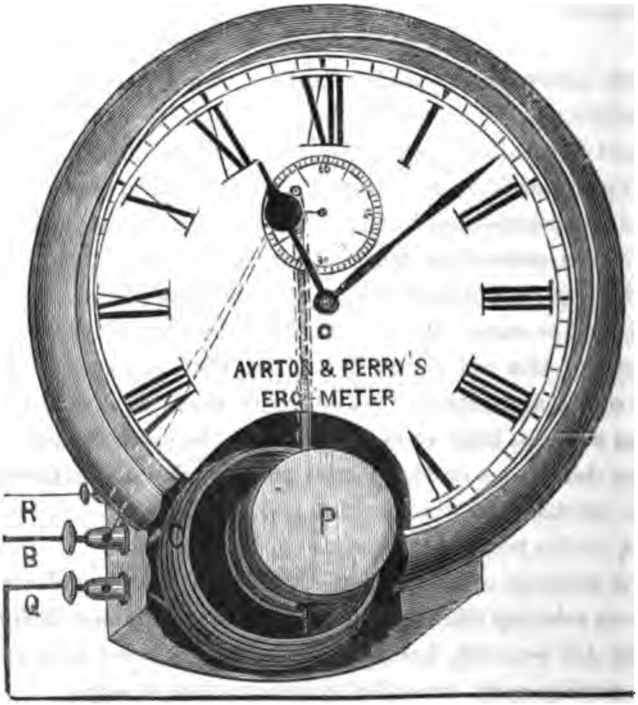
* See *La Lumière Electrique*, May 27 and June 10, 1882.

As in the power-meter, two coils are here made use of. A thick wire one on the main circuit, to measure the amount of current; and a thin wire one on a shunt joining the ends of the main circuit, to measure the difference of potentials or electromotive force of the main circuit.

In the arrangement, as now shown, the thin wire coil, of, say, one 1,000 ohms resistance, simply replaces the pendulum bob of a clock, P. The wires from each end of the coils pass up the sides of the pendulum rod and on to the binding screws, R and B, which can be joined to the supply and return cables of a house, or machine, or a system receiving electrical energy.



In the immediate vicinity of the fine wire coil, and parallel with the plane of the pendulum path, is a thick wire coil, C, fixed to the clock case. This forms part of the main circuit, and has a very small resistance.

The effect upon the thin wire coil of its repeated passages in front of the thick wire coil is to cause a certain pull or attraction upon its motion—either of acceleration or of retardation, according to the direction of the coiling. This acting, in addition to the ordinary action of gravity upon the pendulum, will keep constantly adding to, or retarding, its rate of motion in proportion to the electrical power of the circuit. This pull is the product of the magnetic moments of the two coils, and therefore is proportional to the product of the current and the electro-motive force.

The effects of these repeated accelerations or retardations upon the progress of the clock keep constantly accumulating, and their total amount can at any time be detected and ascertained by observing the amount of loss or gain which the clock has experienced. As the rate of loss or gain in the clock due to different amounts of electrical power has been previously ascertained, this knowledge of the total retardation or acceleration upon the clock is, in fact, a record of the total amount of electricity energy which has been expended, or of the work done, since the last observation of the clock.

The principle upon which the erg-meter is based is as follows:

If c be the current passing into the house, and v the difference of potential of supply and return cable, then $c\ v$ is proportional to the electric horse-power given to the house.

Now the time of vibration of the pendulum is $t = \pi\sqrt{\frac{M}{g-k\,c\,v}}$ if the magnetic force is a retarding one, M, g, v π being constants—that is, if the time of clock, when no electricity is passing, be called T, then $\frac{T}{t} = \sqrt{\frac{g-k\,c\,v}{g}}$.

Now care is taken that the magnetic forces are very small in comparison with gravitation forces acting on the pendulum bob; so that, to a degree of approximation which is sufficiently correct in practice, the above equation becomes $\frac{T}{t} = 1 - \frac{k}{2\,g}\,c\,v$. The rate of loss in the clock is therefore represented by $\frac{k}{2\,g}\,c\,v$. That is, it is proportional to $c\ v$, the electrical power. Hence, the total loss of the clock during any time represents the total electrical energy given to the house during that time.

For the reasons stated in the earlier part of the paper, it is probable that the supply of electricity in a town will be carried out at a certain defined standard pressure or electro-motive force, which will be guaranteed to the consumer; and to assure the fulfilment of this condition special steps will be taken by the local or other authority.

The consumer need therefore but concern himself, as far as the supply he receives and has to pay for, with the quantity of electricity which he has made use of. In fact, the meter he makes use of to record it may be a coulomb-meter. The work, or energy-meters will not be needed for general application, but only in special cases, where the complete record of the total electrical energy supplied is required.

In what form, therefore, will it be most convenient for these coulomb-meters to present their record? or, in other words, upon what unit should that register be based for commercial calculation? With the sole exception of those meters based upon electrolytic action, and which simply present the total of the increments of the metal, which are proportionate to the total quantity of current which is passed, the record presented is the result of a series of mechanical actions indicative of the strength and duration of the electrical supply.

It is seen how these successive mechanical operations may be recorded in a cumulative way by the ordinary arrangement of a train of wheels; both in the case of those meters based upon electrolytic action, and causing periodical reversals in the direction of the current, and also in those where the action due to the current is a purely mechanical one.

There remains then but the question of the mode of graduation of the respective indicating dials.

The unit already referred to, proposed to the Board of Trade in several provisional orders, viz., "the energy in a current of 1,000 ampères with an electro-motive force of one volt flowing during one hour," has been shown to mean 1,000 volt-ampère-hours, or 3,600,000 volt-coulombs.

As it is probable that one of these factors (the volt), being fixed and constant, may be taken out of the commercial calculation for the consumer, it is worth while considering what dimensions the remaining factor (the coulomb) would assume under the ordinary conditions of practice; and hence what would be most convenient graduation of the dials to embrace these dimensions. But the commercial question requires also a financial expression in which to reckon these dimensions.

The money value accompanying the suggestion of the above 1,000 volt-ampère-hours unit, and which is sought to be attached primarily to 100 of these units, is in some provisional orders a maximum of 70s., in others of 75s. Though it would be quite out of place in this paper to attempt to assign any financial value for the supply of electricity, yet for the nonce, and in carrying out the analysis of the above unit, as it might work under the conditions of actual practice, it is necessary to assume a money value. In the annexed table, which takes into consideration the effect of different rates of electro-motive force, according as they may be fixed, 70s. is assumed as the maximum money value for 100 units of 1,000 volt-ampère-hours each.

Comparative Values of Supply of Electricity per 100 *Units (of* 1,000 *V.-A.-hours*) = £3 10*s.*

(1,000 Volt-ampère-hours = 3,600,000 Volt-coulombs.)

E.M.F. Volts.	PRICE.			
	1,000 Coulombs.	10,000 Coulombs.	100,000 Coulombs. (1 Unit.)	1,000,000 Coulombs.
	Pence.	Pence.	s. d.	£ s. d.
100	·23	2·33	1 11·3	0 19 5
110	·26	2·57	2 1·7	1 1 5
120	·28	2·8	2 4	1 3 4
130	·3	3·03	2 6·3	1 5 3
140	·32	3·27	2 8·7	1 7 3
150	·35	3·5	2 11	1 9 2
160	·37	3·73	3 1·3	1 11 1
170	·4	3·97	3 3·7	1 13 1
180	·42	4·2	3 6	1 15 0
190	·44	4·43	3 8·3	1 16 11
200	·46	4·67	3 10·7	1 18 11

The prices in this table are based on the assumption that the price per coulomb unit remains at the same rate, irrespective of the intensity of the electro-motive force. As a matter of fact the amount of the latter ought certainly to be taken into account in fixing the sale price.

From a careful consideration of this table, it would appear, that the amount of 100,000 coulombs would form a convenient unit of value whereby to reckon the commercial price. It would also serve as a basis of graduation for the indicating dials of the meters, with subdivisions to $\frac{1}{10}$ and $\frac{1}{100}$ parts.

$$\begin{array}{r}\text{The values in}\\ \text{C.G.S. units are}\end{array}\left\{\begin{array}{ll}\text{Volt-ampère-hour unit} & = 10^{10}.\\ \text{Coulomb unit} & = 10^{4}.\end{array}\right.$$

The advantage of the coulomb unit is that it expresses an actual quantity, whereas the ampère-hour unit is but a time-rate, which, as a matter of fact, has to be translated into its corresponding quantity.

The coulomb method of graduation could be applied *uniformly* to all quantity-meters, whereon they might happen to be afterwards used, inasmuch as the special rate of electromotive force (or standard pressure) in each particular locality would be taken into account in the price there charged for the electric supply. This uniformity in manufacture would further reduce the expense of this, already the simpler, form of registering meter.

With the volt-ampère method, to use a quantity meter (as shown in the index-dial diagram) requires the exact position of the unit to be set according to the particular electro-motive force with which it is used; inasmuch as the quantity-record is only one factor in the unit. This limited use of each particular recording meter would tend to create error and confusion.

With the volt-ampère-hour mode of measurement an energy meter alone would supply a complete record; but this is a form of meter which involves a clock or some other time record, and consequently is a more expensive apparatus, and one requiring an amount of attention from the consumer, which it would be impossible, in many cases, to expect.

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Thus, although the volt-ampère system may present, *theoretically*, a complete and convenient means of recording the supply of electricity, yet, *in actual practice*, the coulomb system would meet all wants, and be more suited to general application.

In conclusion, it is hoped that one effect of the preceding brief review of the subject of electrical measurements may, perhaps, be to remove an impression which seems to exist in the mind of the general public, that electricity cannot be accurately measured. It is hoped, also, the remarks may tend to point out, that some steps have already been taken to secure a permanent record of a supply of electricity for commercial purposes.

After the conclusion of the discussion, some of the registering meters, such as Hopkinson's and Boys' (vibrating), were shown in operation.

Mr. C. V. Boys: I think I might with advantage describe the arrangement of the integrator which I have applied to the energy-meter. I am sorry to say that at the present moment the energy-meter which I wished to have here has been removed for some changes, but another instrument, in which the same integrator is applied, serves to show most completely what the nature of this integrating mechanism is. A cylinder is allowed to reciprocate, and in contact with its surface is a disc, the plane of the disc being parallel to the axis of the cylinder. So long as that is the case, the disc travels in a straight line on the cylinder, and, as it cannot turn, it does not require the cylinder to turn either. But if, as a result of electrical energy or of steam pressure, the disc is inclined more or less, then in rolling along the cylinder it will attempt to describe a spiral line; but, as it is fixed in position and cannot move at all, the ground, so to speak, on which it is travelling will have to move instead: the consequence is, that through a displacement of the plane of the tangent-wheel more or less, the cylinder will rotate more or less. The instrument before you is intended for steam pressure, and therefore the power of my thumb is scarcely sufficient to make it act, but there is just enough pressure to make the action perceptible on the dial which registers the work of the engine.

The vibrating meter now before you will be shown in action presently, and I do not think it necessary to say much more except perhaps that, theoretically speaking, when a body vibrates under the influence of any force, it is of the utmost importance that the amplitude of vibration should be constant. If we take the case of the pendulum of a clock, it is known, if the amplitude varies and is greater at one time than it is at another, that the clock will not measure time accurately, because the force which brings that pendulum into its middle position is not proportional directly to the displacement, but to the sine of the displacement. If, however, we have a watch in which the balance-wheel is controlled by a hair-spring in which the force is directly proportional to the displacement, then, no matter whether the arc is large or small, the time will always be the same. Therefore, in the vibrating meter, the great point was, either to make with a given current strength the force directly proportional to the displacement, or, not having that proportionality, to have the arc constant. The instrument I have here, and which I hope to show at the close of the meeting, solves the second problem. The arc is practically constant, for it receives an impulse which is itself proportional to the controlling force under which it acts. Suppose you have a clock driven by a weight instead of a spring, and that you take it to the moon, or some region where the force of gravity is different to what it is on the earth, the clock will go faster or slower. If the clock were taken to the moon, where the force of gravity is less than on the earth, and if the same impulse were applied to the pendulum that was applied on the earth, the length of the vibration would be greater —it would not keep proportionate time; but if by taking the clock to the moon we change the controlling force, and the amount of impulse due to the driving force varies in the same proportion, then the clock would keep proportionate time, and the arc of vibration would not vary. This instrument does correspond to the case of a clock driven by a weight taken about from place to place, where the impulse and the controlling force vary in the same way; and it does not correspond to the case of a clock driven by a spring, in which the impulse does not vary. I wished to point that out, as if it was not specially mentioned it might appear that the variation in the force acting on the vibrating armature would produce an error which, as a matter of fact, does not exist.

The **President**: Gentlemen,—This evening's paper shows us that much thought and labour is being bestowed upon current-meters, and happy will be the man who succeeds in inventing a meter combining simplicity with exactness, for no doubt the time is fast approaching when there will be a great demand for such instruments.

We must not lose sight of the fact that but few of the users of the electric light or power will be electricians, and consequently, the meter should be as simple as possible, so that the ordinary householder can understand it, or he will never know what he is paying for. I have given the subject much attention, for it has great interest, but I do not consider myself qualified to criticise any of the instruments now before me, though no doubt there are many gentlemen present who will be able to do so.

Professor **Perry**: I was unfortunately detained on my way here, and was not present when Mr. Shoolbred described the *erg-meter* which has been invented by Professor Ayrton and myself. I have no doubt he has given the gist of the construction and the principle of the action of this instrument, but, at the same time, a few words in addition from me on the subject may be acceptable to the meeting.

The instrument is simply a slightly altered office clock. We find that to take readings of the electrical energy which has been supplied to houses, machines, motors, or accumulators, as accurately as a gas-meter registers the gas energy which passes into a house in a certain time, something like a 30s. clock is needed. With a good clock of that kind, possessing a seconds hand, the constant of the instrument may be obtained in a few hours, whereas a much longer time would be occupied in determining the constant if the clock had no seconds hand. By the constant of the instrument, I mean the amount of electrical energy which is represented by one hour of slowness. The clock hung up on the blackboard before you has simply had its pendulum bob taken off, and a coil of wire of about 1,000 ohms substituted: on the case of the clock, near the fine wire coil, there is a thick wire coil of a few turns through which the main current passes. Such an arrangement does not cost much, and it transforms a 30s. clock into an erg-meter. It is true that this specimen needs to be wound up every week, but such a clock could be made to be wound up every month or every few months, or it may be made self-winding by introducing a little motor worked by the electrical energy supplied to the house. Alterations of this kind are very easily made. Again, it is possible that after twelve hours of slowness in this specimen we may get rather confused in our measurements, but we have not found this a practical difficulty in using it, because we have not required to use it for so long a time as three months, nor, indeed, for more than only a few days or a week at a time. Another slower hand being added would prevent any complication.

In the construction of this erg-meter we have the experience gained by the Clerkenwell clockmakers during the last 150 years. When Mr. Boys read his paper sometime ago before the Physical Society, I told him that I thought it would take some little time (it is now about a year or a year and a half ago) to make the adjustment of the escapement work properly—in fact, to gain in the adjustment of his instrument as much experience as has been gained by our first mechanicians in the last 150 years. His instrument is now, I believe, in pretty much the same state as it was then.

The point I want to make is this:—Here we have a cheap instrument whose construction has been improved upon during a great length of time, and we do not change the delicate parts of it—we simply substitute a coil for a pendulum, that is all. It can be made to measure quantity of electricity or electrical energy, so that it may be called either a coulomb-meter or an erg-meter.

Leaving the construction of the meter behind us, however, I should like to speak of the measurement of electrical energy, or of electricity, generally. I think that probably there is no subject which could be brought before this Society which is of greater practical interest than that of the continual measurement of the electrical energy which passes into houses, and the world is looking to the Society of Telegraph Engineers and of Electricians for some information on the subject. Remember that the world thinks of a current of electricity as it thinks of a current of water supplied to houses. Now there is a very essential difference between the supply of electricity to houses and the supply of gas or water to houses. When gas is supplied, you have only to deal with the *quantity* supplied: you are not going to use gas, I need hardly say, for its pressure energy, and you are really dealing with *quantity* of gas. Similarly, when water is supplied, nobody cares very much about the pressure so long as it is sufficiently high to reach the top story of his house. But if the pressure of water be doubled or quadrupled, or say that a pressure of 700 lbs. to 1,500 lbs. or 2,000 lbs. per square inch is given, then the pressure becomes a very serious matter. Water of such pressure can be used certainly for washing purposes, but this is a very subordinate use in comparison with its use in water-pressure engines, for example. I do not suppose that any of us pay a particularly high amount for a cubic foot of water supplied to our houses; and you may not be aware of the fact that in Hull water is supplied by a company as pressure water. It is not merely water, but water at a pressure of 700 lbs. per square inch. Every cubic foot of water received per minute at that pressure by a consumer means three horse-power. In that case the pressure is of exceedingly great importance, and for measurement purposes you must take the quantity of water multiplied by the pressure. And although mere quantity of water may be important for washing purposes, we know of no purpose requiring the use of mere quantity of electricity: it is needed as a source of energy to drive a motor or to produce light, and the amount of energy obtained for these purposes is what must be measured. The volts multiplied by the ampères and divided by 746 gives you your horse-power. Our supply is, then, one of energy, electrical energy—not merely a quantity of electricity. Nobody cares merely for the quantity of electricity. You want to know what the pressure is, or what is the number of volts, and also what the current is in ampères: the product of these two things is the important point. Now, it would be very well if we could supply all over England, and all over the world, electricity at exactly the same pressure, but as a matter of fact we do not supply electricity everywhere at the same pressure, and I believe it will be very many years before we do. I do not believe that it is possible to supply it: I do not believe that ever we shall, though we may gradually approach a state of things where there will not be any great variation of pressure along a street. The Edison Company at present supply a certain quantity of electricity, and they supply it at 100 or 110 volts. I have been using some Swan lamps which work at about 40 volts, or, let us say, 50 volts for the one and 100 volts for the other. It is quite evident that a certain amount of Edison's current is worth twice as much current from Swan. Edison is now working lamps with 1,000 ohms resistance, and the electricity that goes to supply those lamps will be a great deal more valuable per ampère than the electricity which goes to supply lamps which have an exceedingly low resistance, and therefore I say that we ought not to try after current-meters merely, but for erg- or energy-meters.

To give you another example. I was talking with a friend not long ago, who was rather ridiculing the idea of supplying electricity for working sausage-machines and things of that kind in towns, and he said: "In my town there are several butchers each of whom has a little tap, of only a quarter of an inch diameter, which supplies water of sufficient force to work a sausage-machine, and gives out a lot of power. Why then should we care to have electricity?" I was astonished at the statement, of course, but after careful enquiry—I will not mention the name of the town, although I rather think that its water-rate ought to be looked into—I found that the place is on the road to another town which really pays for the water supply, and those people midway get the water-power, and do not pay for it. They no doubt pay for the quantity of water delivered to them at some small figure, but if they paid for the amount of energy delivered to them, and which is transmitted by the agency of water from engines at a pumping station, they would pay a very much larger sum.

Now I think there is no electrical-energy-meter which is not really a current-meter, or cannot be converted into one. I am not arguing in favour of our own meter, because I can call the instrument hanging up there a coulomb-meter and make it merely measure quantity of electricity, but I am referring to Mr. Shoolbred's final remarks, and would say that there is no great difference between an erg- and a coulomb-meter in construction. You must have a clock arrangement in either case: the one must be just about as simple as the other in any case. I say that we must have an energy-meter and not a current-meter, even after perhaps ten years have taught us how to make the pressure exceedingly uniform all over a town. Those of you who attended Professor Silvanus Thompson's recent lectures at the Society of Arts know that there are various methods (in some of which Professor Ayrton and myself are interested) by means of which two points can be kept at a given difference of potential, no matter how many lamps may be worked from those points. It has been arranged that in, say, a distribution of incandescent lamps, if you have a steam-engine with a good governor, and a dynamo running at a fairly constant speed, it is quite possible to keep the terminals at a given difference of potential, and that if a single lamp were kept burning immediately between them it would burn with a constant brightness whether a thousand lamps were in use or none [Professor Perry drew a sketch on the board to illustrate this]—whether you shunted off suddenly 999 lamps or not, the same brightness is maintained in the one lamp. But that is not the case in practice with every lamp, and I do not see how it can be worked out easily. I do not see how the potential at an intermediate point can be kept exactly the same at all times, because you certainly have a variable current, and therefore certain differences of potential must be established. It is therefore difficult to see how you will supply electricity to houses at a perfectly constant pressure. I do not know whether any member has worked the problem out and sees his way to it, but until we get electricity supplied at a perfectly constant pressure to all houses, it is a most unfair thing to charge by the mere number of coulombs of electricity going into houses.

Mr. R. E. CROMPTON: Professor Perry, when he said that it is absolutely necessary to measure what I may call the electrical pressure, as well as the current, is quite right, when the question is viewed from the Electric Companies' side; but I am afraid that the public, on the other hand, the interests of which are supposed to be protected by the Board of Trade and the local authorities, will not take this view. I am afraid they will insist that we should supply the current at a fixed electrical pressure, and make out our account for so many ampères of current supplied at this fixed pressure, so that even if we are told that we are to charge by energy, the pressure being thus fixed, it will be only necessary for us to measure the current. Therefore these beautiful energy-meters are unnecessary as energy-meters, and may be simplified down, as Professor Perry proposes, to be current-meters. I am not aware that the difficulty that Professor Perry speaks of as to the varying pressure at different points, on a long main from which many branches are being fed, is at all an insurmountable one. You will observe that in all the provisional orders we are allowed to vary our pressure about 10 per cent., that is, 5 per cent. on either side of the standard pressure. This might be a very serious matter if we had to name one fixed standard pressure for the entire length of main, but this we are not called on to do. We can name the standard pressure for several points on the main, and this removes a certain portion of the difficulty. The arrangement of electrical mains so as to minimise this difficulty has received my close attention, but you must allow me not to describe this at the present moment, although I promise to do so at another date. I may mention the difficulty is not so great as it appears. Practically, we have succeeded in working an installation of considerable size within very reasonable limits of pressure. I have one large installation now at work where there are 500 lamps fixed, the length of mains and branch wires being very considerable. The standard pressure is 59 volts at the lamps, and the extreme limit of variation is from 57 volts when the full number is on to 60 volts when there is only one or two on, and this without any attention to the speed of the engine, which, having a good governor, maintains a very constant speed throughout. You will see that the extreme variation in the above case is only about 6 per cent., which is less than the variation which the provisional orders contemplated. I think that when we get to large installations, in which we shall have the aid of electric governors to so control the steam-engines that the pressure of the mains at the dynamo terminals will remain practically constant, we shall then be able to work well within the limits of variation from the standard pressures as allowed by the Board of Trade under any ordinary conditions as to sudden demand for current, etc.

I do not think we shall be allowed to change our standard pressure without long notice—in fact, notice long enough to enable our customers to use up their stock of lamps, etc., without any great inconvenience. You must recollect that an incandescent lamp is really a very practical means of measuring difference of potential or electrical pressure; and although some one has said that "we must not expect the users of the electric current to be all electricians," yet I believe they will all be able to estimate the difference of pressure between 100 and 110 volts, for the increase or diminution in the brilliancy of their lamps will be a sure guide to them in this matter.

I shall be glad to see these wonderful Edison lamps referred to, which admit of the use of such high pressures, but they are a long time coming.

There is no doubt whatever that our present lamps, which reach as a maximum 120 volts, are of too low resistance; and, doubtless, in the immediate future 200 volts will be the limit, as suggested by the Committee of this Society, which has had under its consideration the subject of fire risks. This resolution of the Committee will govern the lamps for some time, but there will be no great improvements in them until a considerable advance is made. Therefore those remarks of Professor Perry, although very interesting, if we have a system of mains for power only, do not apply when we have in contemplation a system of mains for lighting as well as power; for there is no doubt but that the lighting will be by far the most important use of the electric current for some time to come. The public have yet to be educated to its use for motive-power. At present it is only in the working of small machines, such as sewing-machines and sausagechoppers, that the public see a possibility of using distributed power; but before many years have elapsed I think we shall see a great many more important uses for electrical power. It is impossible to overstate what may be done in the future, but there is no doubt that when the distribution of power comes to take its true position in comparison with the distribution of current for lighting purposes only, we shall be compelled to use currents of far higher pressure, and vary this pressure to a far greater extent than we propose to do at present. We must hope then to have to deal with a Board of Trade and local authorities better educated in these matters than they are at present. But, as it is, we must rest and be thankful for some time to come with the limits of pressure we have under great difficulties secured in the provisional orders that have been thus applied for. I say secured, but I must speak under correction, as these limits are not even yet finally settled.

Professor **Perry**: I agree with Mr. Crompton that measurement of energy rather than current will be more important when machinery is worked by electricity in houses as well as lamps, but he is too favourable to my view of the case when he states that "we shall in the future regard ourselves as having been duffers in the *current* year," that is, in the year when we charged by *current*. That future time is rapidly approaching. I have been told, on good authority, that in the district which is being lighted up by the Edison Company in New York there are 2,000 motors driven by the system actually in operation; and my informant says that the Edison Company regard the electric light as altogether a waste product, because their supply of power was going on all day long, while the electricity for lighting purposes was only given out during a certain interval in the evening.

Major **R. Y. Armstrong**, R.E.: It is so very late, and the meeting has heard so clearly all about these different meters, that I do not propose to take up its time any longer. I would merely ask Mr. Crompton whether, in the views he expressed on the point of raising the difference of potential to limits which he did not mention, he proposes to dimish the sensitiveness of the human frame to the action of these differences of potential?

And I would ask Mr. Shoolbred, when he suggested that a pressure of 200 volts should be charged for only, say, at two-thirds of the price of the same quantity supplied at 100 volts, whether he has taken into account, or has got any data as to the deterioration of the cables caused by the supply of electricity at 200 volts? So far as I am aware, experience of working at this pressure is limited. There is a great deal of experience in telegraphic pressures, but they do not reach 200 volts; and what the life of a cable treated continually with 200 volts will be, is, I imagine, not within the knowledge of anybody.

Mr. KILLINGWORTH HEDGES: Mr. Crompton has told us how the Electric Companies are prepared to treat the public: it seems to me that we ought also to consider how the public are prepared to treat the companies. Those meters, in my opinion, are open to the objection that they can be tampered with by the scientific members of the public. I should like to ask the gentlemen interested in these meters whether they have taken any precautions to prevent them being influenced by magnetism? Suppose, in the case of the vibrating meter, that an electro-magnet was by some means brought to bear upon it, would its influence stop the vibrations? or, in Professor Perry's meter, would it stop the hands of the clock? or, in another case, the armature? In a gas-meter its very safety lies in the danger of interference, so no one cares to try experiments with it: this is not so in a rotary electric meter, as the mere effect of heating the bearings would make it run slower, and cause it to record less current.

Another point that occurs to me is, that an enterprising electric light user may find a way of stealing electricity if we do not look out. He may take a wire and connect it to the main before it goes to the meter, and to this branch wire connect the lamps he may wish to light without paying for. I do not see that the meters provide for such cases, and the only plan would appear to be to employ a safety plug, which might be arranged so that any excess of current passing beyond what was necessary would fuse it and detect the theft. I have experimented with many lead plugs such as Edison's, but have been unable to get one to melt satisfactorily with a given current. The ordinary plan is to allow a margin of 50 per cent. between the melting point of the plug and the working point, which would allow 40 per cent. of the current to be stolen; but if something could be got out to melt within, say, 5 per cent. margin of the necessary current, it would be a better check on the consumer. My experiments lead me to suppose that there is no difficulty in doing this with an improved form of "cut-out," which, when perfected, I shall be pleased to submit to the Society.

Professor **Perry**: Is it not possible to tap an ordinary gas-pipe beyond the meter? Is it not possible, also, to take a gas-meter and tilt it slightly, and so change the readings? The fact of the matter is, that the temptation is open to a scientific man to do many such things, but the scientific man is an honest man, I think, as a rule, and will not use his knowledge for dishonest purposes.

Mr. C. V. **Boys**: I have been frequently asked about the probability of these meters being tampered with. Of course, when it is said that the meter depends upon the movement of certain mechanical parts, it is naturally thought that if a magnet is brought near it its influence will be brought to bear, and, therefore, that the meter can be easily tampered with. The two instruments which Mr. Shoolbred has described, the energy-meter and the vibrating meter, are not open to this objection. I will take the energy-meter first. The only movable portion of it consists of an equal number of turns in the movable part in opposite directions. It is true that if you could probe a magnet right into the meter by boring a hole in the case, then the action on one-half of the movable portion would be very different to what it would be on the other half. But if a magnet were put outside the case of the meter, its action on the movable portions would be equal and opposite, and there would be no effect.

In the case of the vibrating meter, the only parts that are made of iron are the small armatures, which are entirely enclosed within the electro-magnets, which are very massive. If you put the whole instrument in a magnetic field, or put a magnet near it, the effect produced is that the iron electro-magnets screen the armatures just as a conducting body connected with the earth screens anything from the action of neighbouring charged bodies. In neither case is tampering possible with fixed magnets.

Mr. J. **Munro**: It seems to me that a thick soft iron case would effectively protect these meters from the action of an external magnet. In the Thomson marine galvanometer the iron case protects the needle from the ship's magnetism, and I think that an inch plate of soft iron would screen most of these meters from the influence of external magnets.

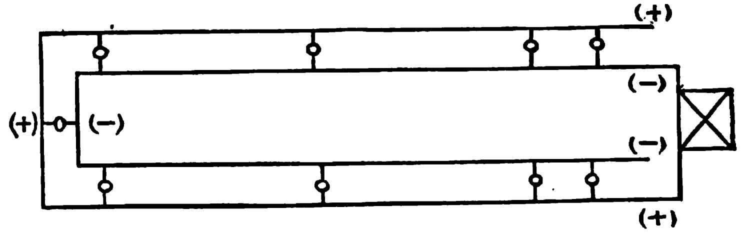
Mr. **Thomas Frazer**: As a stranger, I should like to make one or two remarks. I think it is quite evident that in supplying a town or house with electricity, as Professor Perry has said,—and I know many others are of the same opinion,—there can be little doubt but that ultimately it will have to be the energy and not the current that shall be measured. And as the energy can be measured with no greater difficulty than the current itself, there is good reason for charging upon the energy, or the real value that is given out electrically to any house, establishment, or town. I do not think that there ought to be any difficulty either in the selection of the unit of work. Inasmuch as the expression E C represents kinetic kilogrammètres, it would be the easiest thing in the world to establish as the unit the kinetic kilogrammètre.

Professor **Perry**: It is 10 volt ampères, I think, that represent one kilogrammètre.

Mr. **Frazer**: That is simply obtained by dividing by G in mètres, and is one kilogrammètre force, therefore the statement holds perfectly true, and you may charge by *kinetic* kilogrammètres absorbed. Energy should be measured, and not current, as it is necessary that you should know what you supply for the money charged, for your own satisfaction. If you simply measure current, let us suppose you have a system in series, that is, with houses or the town supplied by one cable, with a constant current of, say, 1 ampère or 10 ampères or 1,000 ampères. If that current of a given number of ampères went through my house, in which a current-meter was fixed, of course it would show the number of seconds, minutes, or hours that I had used it, but that would not show how much work I had done with that current. It would n show you whether I had burned one lamp or ten lamps. It would not show whether I had thrown down 1 oz. or 1 lb. or 10 lbs. of metal; for if I sent it through one tank, I should, according to the resistance of that tank, do a certain amount of work—if I sent it through more, I shall increase in proportion the work done. If I used the current for *n* hours, I should simply have the *n* ampère-hours to pay for, and therefore you who supplied the energy to me would charge for less than the supply.

Again, as to working with different electro-motive forces: that is a very ticklish point. It certainly would never pay to lay down cables for one electro-motive force and then to use a considerably lower one. On the other hand, it would not pay either to use too great an electro-motive force with a given cable. We have companies who work all over the kingdom at potentials which, in my opinion, ought to be prevented by Act of Parliament. I do not consider that it is at all safe to work at 2,000 volts as is at present done, with what results you already know.

Mr. Alexander Siemens: I just want to make a few remarks in reference to the current-meter, not that I am in any way opposed to energy-meters, because I think that form is a good one. But the lighting of a large section of a town is simple in one respect. We must give all the people the same difference of potential, because, although they have the option, in practice they will all select one kind of lamp, and there is no such difficulty, as has been anticipated, of the difference of potential varying in different parts of the circuit, as a simple alteration in the connections will obviate it. The connections should be made as shown in sketch.



By that means all the lamps are at the same difference of potential, and therefore it would be perfectly practicable to find out by official tests what difference of potential is used in a certain system, and then simply to introduce current-meters for determining the amount of energy.

Mr. H. C. **Granville** asked Professor Perry if he did not fear that the clock meter would not require a clock of absolutely uniform motion; for, since the energy depended on the loss in the rate of the clock, if the clock itself lost or varied in its rate, an error would be produced in the calculation supposed to give the result of the energy consumed. Such a uniform clock was practically impossible, and therefore a source of error would exist in the instrument.

Professor **Perry**: The clock would have a certain amount of error, and I think I gave the indication of that error when I said a 30s. clock would do. We have gone into the matter, and have really found that for our own practical working, but only working up to not more than two hours loss of time per week, the error in the case of a 30s. clock was not of such importance as the ordinary error in a gas-meter. A 30s. clock will go fairly well, and will not lose more than two minutes a week, and this loss, compared with about two hours a week is quite an inconsiderable source of error.

Mr. J. N. **Shoolbred** said, that most of the questions as to the construction of the meters had been answered very fully by Professor Perry and Mr. Boys. There was one point, however, that should, in justice to the remarks in the paper, be pointed out, particularly after what had fallen from Professor Perry. In the paper it was clearly stated, that the energy was the basis of the evaluation of the supply, and that the *total energy* must be taken into consideration. By the use of the coulomb-meter and unit of graduation, it was never intended to advocate, that the electromotive force should be left out of the calculation. But, since in most cases the authorities (as they did at present with gas) would see to the observance of the prescribed pressure, why should the consumer then be forced to adopt a more expensive and complicated form of meter, in order to take into consideration a matter which was already allowed for in the price of his supply? It was to be hoped that, as the importance of the subject had been urged by several speakers, further attention might be drawn to it by the proceedings of that evening, and that what had been then commenced might lead to a careful and complete solution of the entire question.

The **President**: We have had from Mr. Shoolbred a most instructive historical paper. It has brought us along the road and left us at a certain point, and I am sure it is very gratifying to see what progress has already been made.

Professor Perry has told us that a 30s. clock can be converted into a meter for electrical energy or current; and Mr. Crompton mentions that he has an engine which requires no attention, and will keep up in the dynamo machine a steady tension of 57 volts.

As to the remarks of Major Armstrong in connection with the life of a cable, that would depend upon its conductivity. If you will only set the conductivity to the amount of current you wish to use, there is no reason why the life of a cable, used for electric light or power purposes, should not be as long as an ordinary submarine or other telegraph line. The hour is late, and I will add no more, except to ask you to give a cordial vote of thanks to Mr. Shoolbred for his kindness in bringing all these facts before us, and also to thank Mr. Boys and others for the assistance they have rendered Mr. Shoolbred by lending him their instruments.

A vote of thanks was heartily accorded.

The meeting then adjourned until March 8th, 1883, when, as announced, a paper will be read by Mr. J. Munro on some new forms of telephone transmitters, with a note on the action of the microphone.

The One Hundred and Twentieth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 8th March, 1883—Mr. WILLOUGHBY SMITH, President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of transfers and new candidates announced and suspended.

The SECRETARY announced the receipt of donations to the Library of the Society from the following:—Lieutenant Bagnold, R.E., M. N. de Kabath, M. Angelo Fahie, Dr. Tommasi, Messrs. Eger & Brander, Mr. C. J. Wharton, Dr. Spottiswoode, and Mr. A. L. Ternant.

A unanimous vote of thanks was accorded by the meeting to the donors for their contributions.

The following papers were then read:—

NEW TELEPHONE TRANSMITTERS.

By J. MUNRO, Associate.

Mr. President and gentlemen,—It was during the well-known telephone trial of last summer* that I began to think of getting out a new transmitter. In order to be a novel instrument, and avoid clashing with the Edison and other patents, it was necessary that a new transmitter should fulfil certain conditions; and my connection with the trial showed me what these conditions were.

The broad claim of the Edison 1877 patent is for the combination of a tympan, or drum, with a "tension," or, as I would rather call it, a "current" regulator of semi-conducting material. The particular material used by Edison was lampblack, a form of carbon; but the claim has been held in courts of law to embrace all kinds of carbon, and might even be extended to all so-called "semi-conductors."

Since the memorable discovery of the microphone by Professor

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* The United Telephone Company *v.* Harrison Cox Walker.

D. E. Hughes, a great many patents have been taken out in England and abroad for carbon transmitters of one form or another; but the judicial supremacy of the Edison patent has in general paralysed these efforts, and rendered the improvement worthless. It is even doubtful if the substitution of protosulphide of copper for carbon by Mr. Moseley, in a recent transmitter, will be clear of the Edison claim; since that salt is a semi-conductor.

I determined, therefore, to keep clear of semi-conductors altogether, and if possible, too, of tympans. The transmitters which I have the honour of bringing before you to-night are made of metal—that is to say, the current-regulator is formed of conductors, so arranged as to render a tympan unnecessary.

Some experiments I had occasion to make in the course of the telephone trial showed me the promise which lay in metals as a material for transmitters—a promise first discovered by Professor Hughes in his microphone researches, but neglected by electricians because of the apparent advantages of carbon. I did not believe that the action of the microphone was due to diminution of resistance at the points of contact by pressure, causing greater or less area of surface contact, according to the current theory at that time. It seemed to me that the film of air between the contact points played an important part in its action, and that the electrodes of a microphone might be of any conducting substance—for example, metals—without destroying its peculiar power of transmitting sounds. I thought, in short, that the current passing through the air between the loose contacts made the sensitive part of the microphone, and that sound-waves traversing this film of air would transmit themselves whether the electrodes were conducting or semi-conducting, carbon or metal.

What kind of metal to use and what particular form to give it, in order to get good articulation and durability of action, were questions which only experiment could answer. In working out any ideas on the subject, I had an able colleague in Mr. Benjamin Warwick, whose zeal in these experiments has been unflagging.

One of the first ideas tried is shown in Fig. 1, and, though it proved a failure, it was instructive in one particular. A B is a fine short platinum wire connected between the poles of a battery, and carrying a sliding contact or chariot, C, which is connected by a light but stiff stylus, S, to the centre of a mica drumhead or tympan. On speaking against the tympan in the direction of the arrow, the stylus shoots to and fro like the recording pen in Barlow's logograph, and slides the chariot, C, up and down the wire to points of greater or less potential. This chariot is connected to one end of the line, which has a receiver at the other end. When there is no circuit through the line, and the device acts by varying the potential of the transmitting end of the line, a Dolbear receiver or a condenser would be used; but if a shunt circuit is formed between the chariot, C, and one pole of the battery through the "primary" of an induction coil, while the "secondary" is in circuit with the line, a Bell receiver may be used.

On trying this plan, however, we found that the current varied too regularly, owing to the chariot always moving in contact with the wire. The strength of current was thus varied by easy grades, after the manner represented in Fig. 2. It was not quickly and abruptly changed, as it needs must be in a good transmitter, so as to render all the sudden irregularities of speech vibration shown in Fig. 3, which is copied from a tracing made by Mr. Barlow's logograph in this hall some four years ago.*

Tried with a tympan of mica or ferrotype iron, placed horizontally, and having a short angular stylus projecting upwards from its middle and rubbing up and down a short platinum-iridium wire in circuit with 7 Bunsen cells, imperfect speech was heard in a Bell telephone connected between the stylus and one pole of the battery; but it might have been due to the microphone contact between the stylus and wire. A mercury drop enclosing the wire to act as a chariot did not give articulate sounds. Neither did the oscillation of mercury up and down a wire in a capillary tube.

The sudden rise and fall, the sheer descent of current, which is required for the transmission of speech, could not be given by such a device.* The microphone, however, gives it, and, as we shall see, this requirement has an important bearing on the theory of that instrument.

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* See *Journal of the Society of Telegraph Engineers*, Vol. VII., p. 66.

The use of wires in these experiments led me to the idea of employing two light wire gratings resting on each other under slight pressure, and forming a metal microphone with multiple points. It seemed that the light open structure would allow the sound-waves to agitate the contacts without the intervention of a tympan, and something after the manner of an Æolian harp.

Fig. 4 is a transmitter made on this plan. It consists externally of a wooden box, B, having a mouthpiece, M, closed by a piece of match-wood. Inside the box is the microphone, or "current-regulator," which consists of a piece of ordinary iron-wire gauze, *g*, lightly pressed against another piece, *g′*. The back piece of gauze *g′* is fixed to the back of the case, but the front piece, *g*, is carried by a loose axle, *a a*, supported in bearings at each end. From the axle projects an arm, carrying a movable counterweight, W, and the downward pull of this weight, tending to lift the front gauze off the back, is balanced by the force of an adjustable spring, S. The pressure on the microphonic contacts is regulated by this means. The stops, *p p*, serve to limit the sidelong play of the axle carrying the movable gauze, when it is found convenient to shift the position of the latter in adjusting the instrument.

The current enters and leaves the regulator by the terminals, *t t′*. A small induction coil (not shown) is usually inserted between the transmitter and the line, the "primary" of the coil being in circuit with the battery and the regulator, while the "secondary" is in circuit with the line wire and receiving telephone.

On speaking into the mouthpiece in the usual manner, the air-waves set up by the voice pass through the two gauzes, and agitate the contacts between them, thereby modifying the current so as to make the telephone repeat the words. The match-board diaphragm is not an essential, as the sound-waves act directly on the contacts, but it is useful in screening the regulator from the rudeness of the breath, and fixing the distance at which the speaker should keep. A grating across the mouthpiece serves almost as well. To show that a tympan is unnecessary, I have here a gauze regulator mounted in an open frame shaped like a lyre, and on speaking to the wires the sound-waves agitate the contacts and transmit the voice. In another form which we have constructed, the sound-waves are reflected upon the regulator by an acoustic mirror; and in a third form the regulator is carried by a sounding-board. The sonorous properties of the case may also be improved by the use of sounding-posts.

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* This objection would also apply to Mr. Elisha Gray's "water dipper" and Professor Bell's "mercury and carbon dipper."

In fact, there are many details of construction which I need not take up your time in describing. The pressure between the electrodes can be regulated by means of magnetism, as well as gravity or the tension of a spring, especially when, as in this case, the metal employed is iron. The pole of a permanent magnet presented to the gauzes will hold them together with a force depending on the nearness of the pole. When an electro-magnet, E, is used as in Fig. 5, the current itself may traverse the coil of the magnet, and hold the gauze *g* against *g′* with a force which will be stronger when the current is stronger, and thus enhance the pressure of the sound-waves, so as to give a more decisive action. For since increased pressure of the sound gives increased current through the regulator and electro-magnet, the latter will react on the gauze so as to intensify the pressure and accentuate the voice. The electro-magnet, E, may be the core of the induction coil used with the transmitter.

In his original paper on the microphone, Professor Hughes states that the microphonic contact between two pieces of metal should be clean, but we find that the gauze may be either clean or tarnished, provided the rust upon it is not so thick as to make the surfaces adhere to each other.

Two pieces of gauze are not essential to the transmission of sounds, as one piece resting on a flat metal plate, either plain or corrugated, will answer; so also will two thin metal plates with roughened surfaces. But gauze is the most convenient and successful material we have yet tried. Its sensitiveness to tremors is remarkable, and it possesses a flexible elasticity, when properly adjusted, which a continuous diaphragm does not seem to have. The numerous points of contact which it offers to the current, its low internal resistance, its lightness, portability, and cheapness, are all in its favour as a telephone transmitter.

Another type of metal transmitters is shown in Figs. 6, 7, and 8. There B is the case, as before, with a match-board screen, M, and behind it a flat sounding-board carrying a metal microphone, *i*, with its adjusting spring, S. An induction coil, D, having its "primary" in circuit with the microphone and battery, and its secondary in circuit with the line, is also contained in the case. The microphone is formed of a light metal bar, *i″*, resting by its ends in holes drilled in two metal blocks, *i i′*, Figs. 7 and 8. An adjusting screw, *s*, regulates the pressure between the bar and its bearings. The metal found most suitable is common cast-iron, which may either be clean or slightly tarnished.

On speaking into the mouthpiece the vibration of the sounding-board causes the iron contacts to transmit the voice. A little clock oil applied to the bearings serves to remove the "boiling" or "buzzing" sound which is apt to spoil the action of the transmitter. Clock oil is chosen because it does not freeze in winter, and because it will preserve the iron from further rusting; but other liquids are found to answer the same purpose.

The sounds given by the cast-iron is loud, clear, and buoyant, but the proper adjustment of this form of transmitter is not so easy and abiding as that of the gauze type above described.

A third type of transmitter is shown in Fig. 9. In this the current-regulator consists of metal granules, *m*, enclosed in a box between two metal plate electrodes, *e e′*, connected to the terminals, *t t′*. The back of the box may either be of wood and the front of wire gauze, or both may be of sonorous wood. Iron or brass filings and turnings serve as metal grains, especially those with a good deal of inherent resilience, or spring; but a pile of small ¼-inch screw nails, or a grit of "spongy iron," give better results. Spongy iron, having a rough surface, approximates to carbon in its action. To reduce the boiling or buzzing sound caused by the continual trembling of the particles, we sometimes immerse them in a liquid, such as alcohol or thin oil. The liquid must not be viscous, else the vibration of the particles is clogged, and articulation ceases. Castor oil, for example, or glycerine, reduces the transmitter to silence. The removal of the buzzing in this way always appears to be at the expense of loudness in the articulate sounds. The steadying effect of the liquid on the delicate microphonic contacts is not its only influence, for, besides acting as a fine vibrator, it preserves the contacts from fouling overmuch, or from sticking together in the way investigated by Mr. Stroh.*

I may add that I have found some kinds of writing-ink transmit speech when held in a box like that described.

A fourth type of transmitter is that shown in Fig. 10, where B is a box, as before, with a mouthpiece, M, and an induction coil, D. The mouthpiece in this case is closed by a tympan connected at its centre to a metal chain, or strip of chain mail, *c c'*, which is strained by the springs, *s s'*, and the tie, *f*. This chain is the current-regulator, and is traversed by the current. Between every two links there is a microphonic joint. The vibration of the tympan, on speaking in front of it, tightens or slackens the chain, thereby operating the current-regulator and transmitting the sounds. The pressure between the links, which by preference are of iron, is regulated by adjusting screws.

Apart from their value as practical instruments, whatever that may turn out to be, these transmitters are instructive in a scientific sense. They show that metal contacts are quite capable of transmitting speech, and that, contrary to the doctrines of more than one eminent scientist, carbon has no monopoly of articulation. There has been a tendency of late to minimise the great discovery of Professor Hughes, and regard the microphone as a mere variety of the Edison carbon transmitter. Sir William Thomson himself appears to share this view, for he has stated, under examination, that he does not think the discovery of the microphone was any important advance beyond the carbon telephone. The part played by metals in the microphone, a part undreamt of until Professor

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* See *Journal of the Society of Telegraph Engineers*, Vol. IX., p. 182.

Hughes' discovery, has been hitherto ignored. In point of fact, however, it is more than half of Professor Hughes' discovery, as these metal transmitters demonstrate. For the carbon microphone is now shown to be only *one* illustration of Professor Hughes' wide discovery, that a delicate contact between any two *conductors* has the property of transmitting sounds. The Reis original transmitter, with a delicate platinum joint, is seen to have been another illustration of the same phenomenon. Professor Silvanus Thompson has recently presented proof that Reis did send articulate words by his transmitter; and I believe there is no doubt he did. I have myself heard by telephone, and understood, single words spoken direct to the platinum contact of a Reis transmitter. Reis, then, employed a platinum microphone, without knowing it, as early as 1861; and Edison used a carbon microphone, without knowing it, in 1877. The discovery of Professor Hughes, in 1878, threw a flood of light on both inventions.

The action of tarnish was probably not observed before the experiments of Mr. Warwick and myself, as both Professor Hughes and Herr Reis were careful to have clean contacts. Even Mr. Edison, in his long catalogue of materials claimed for his transmitter, and numbering nearly all known substances, as well as several which remain to be discovered (either in this world or the next), does not, I believe, mention rust.

I have some working specimens of the transmitters on the table, for the inspection of the meeting, together with samples of the gauze, spongy iron, and other materials employed in making them. Two of the wire gauze transmitters are also connected up with telephones between the council room below and the lecture hall, in order that members may have an opportunity of hearing them speak at the close of the proceedings.

I have another and supplementary communication to make on the theory of the microphone, but I would rather defer it until after the discussion on these apparatus.

SUPPLEMENTARY NOTE ON THE ACTION OF THE MICROPHONE.

My first intention was only to bring the metal microphone transmitters to your notice this evening, but perhaps something might be said in addition on the current theories of the action of the microphone from the point of view of these transmitters.

The original theory of Edison, that it is due to a reduction of the internal resistance of carbon by pressure, is probably not maintained any longer, in face of the numerous experiments showing that there is exceedingly little reduction, if any. Then there is a theory, supported, I believe, by Sir William Thomson, that the action is due to an increase or diminution in the area of surface-contact produced by increase or diminution of pressure. But how the area of contact is to vary in simple ratio to the surface-pressure is a puzzle in this theory which is difficult to see through. A third theory, due I believe to Mr. Preece, is that the microphone acts by heating at the points of contact when the current passes. I have not seen this theory developed anywhere, and may not understand how the heating is supposed to act; but if it acts by altering the resistance of the points, then in the case of carbon we have a diminution of resistance, and in the case of metals, more especially iron, a rise. Yet, as we have seen, metal points, as well as carbon ones, act as transmitters. The theory, then, does not seem to fit both classes of instruments. Again, it does not seem probable that the variations of temperature can be so quick and sudden as the transmission of sonorous currents requires. The logograph curve shows that a jerky action is necessary, and the heating and cooling of a body must be more or less gradual. Another reading of the theory may be that the heated points are of a spongy nature, and more sensitive to pressure; but here again it is difficult to see how the effect can be in simple ratio to the pressure. A fourth theory, supported by Professor Hughes and Professor Blyth, and probably first intimated by Mr. F. H. Varley,* attributes the action of the microphone to a small arc formed between the points, and this appears to me a likelier hypothesis. But I would propose a modification of it, and, instead of an "arc," in the ordinary sense, consider the cause a "silent" discharge through the film or stratum of air or liquid between the points, after the manner of a lightning guard. With a strong current one can see sparks passing between the points of the microphone, but this momentary arc is due to excess rather than to normal action. The normal action is evidently produced by a silent discharge going on between numerous tiny projections on the carbon points, which in this respect resemble the corrugations or spines in two well-known forms of plate lighting arrestors used in telegraphy. We have to figure to ourselves the microphone points covered with irregular projections which do not touch each other, but are bathed in the surrounding air and ether. These projections, like the points of a lightning arrestor, are constantly discharging little jets of current from one to the other across the air between. But the action of the sound-waves is to move them to and from each other, thereby increasing and lessening the width of the air stratum, and varying the strength of the discharge. When this vibration is clogged by a viscous liquid, the sounds are muffled and indistinct.

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* See *Electrical Review*, Vol. VI., p. 404.

The "boiling" or "buzzing" of a metal transmitter not properly adjusted reveals this "silent" discharge from a multitude of points. The ear hears in the telephone a peculiar soft noise, quite indescribable, but apparently made up of a million feeble ticks, the feeblest the ear can appreciate. Each tick seems to be caused by an infinitesimal discharge between two little points, and the agitation of these points causing the discharge is the perpetual tremor going on in all substances, especially elastic metals. A slight pressure applied to the microphone quiets this discharge and leaves the sonorous vibrations to act by themselves in disturbing the flow of electricity through the air. It silences the buzzing of the crowd, as I may call it, and allows the voice of the speaker to be heard.

If the sonorous waves are too violent, the points are separated too far, the contact is broken, and a sharp click is heard in the telephone. This is due to the stoppage of the discharge, and is comparable to the "click" heard by Mr. Stroh in the experiments referred to, when the current was stopped in the metal contacts. It is the sound heard on a larger scale on stopping the current in an arc lamp. It seems to me an effect of the rarefied arc rather than the heated points; for if it were due to shrinking of the points it would be observable in an incandescent wire on stopping the current through it. My opinion is that the rarefaction of the air and ether caused by the discharge is the true source of this effect. When the current suddenly ceases, the rarefied gas collapses, and equilibrium of fluid pressure is restored. Of the attraction caused by the discharge between the points, I do not now propose to speak, further than to point out that it undoubtedly exists, and that, just as the two carbons in an electric lamp tend to come together, so doubtless do the two electrodes of a microphone. It is perhaps caused by the reduction of ether pressure between the points. Mr. Stroh attributes the sticking of electrodes to heat and welding; but this feeble electric attraction has probably been overlooked.

I should like the President to hear this peculiar buzzing sound and the click caused on breaking contact. I have therefore arranged a microphonic contact between a piece of iron wire gauze and another piece of copper gauze. The contact is connected in circuit with a single cell and a telephone. On agitating it a buzzing sound is produced, and on breaking contact altogether a click is heard. Only the other day I found, as I expected, that heating the contact by a spirit lamp increases the buzz, or breathing sound, by facilitating the discharge. [Experiment shown.] Thus, when I apply the lamp flame to the contact, the telephone, before silent, begins to sigh, with increasing loudness, and gradually becomes silent again when I remove the flame and allow the contact to cool.

With two pieces of iron wire gauze, one delicately resting on the other, I have sometimes obtained this breathing sound very perfectly without the application of heat. In loudness it resembled the rushing of a strong wind. The slightest oscillation of the upper gauze until it came to rest was rendered in the telephone. Even the periodic tremors of my hand, in contact with the table, were distinctly given, and broke up the blast like the puffing of an engine.

The application of heat, by the spirit flame, to two pieces of wire gauze in contact, increases the microphonic action between them in a similar manner, as can be shown by placing the spirit lamp below such a contact. When the flame is applied to the metal so as to heat the contact, the tapping of my hand, as heard in the telephone, becomes louder, and gradually dies off when the flame is removed. Hot metals and flame are known to discharge electricity better than cold metals, and hence, in all probability, the slow discharge of the microphone is accelerated by this means.

Heating the contacts can of course be applied to the metal transmitters which I have shown, especially the gauze forms; but I have not yet had time to properly try this plan. The superiority of iron in these transmitters may be due to the ease with which it discharges, as compared with the other metals tried; but heating facilitates the discharge of other metals besides iron. A transmitter of platinum wire gauze, which would not oxidise in the flame, as does copper, for example, might therefore be constructed; the spirit lamp being fed from a reservoir attached to the case and lighted at will by an electric current sent through a platinum wire on touching a press-button, or in the act of unhooking the telephone receivers from the case in order to converse.

When the microphonic contact is formed by two different metals, the application of heat generates its own current by thermo-electric action. I have here a contact formed of iron and German silver, and on applying the spirit lamp, and tapping, we hear the taps in the telephone. In Dr. Mathiessen's scale of thermo-electric effects, bismuth ranks 25, German silver 5·2, and iron 5. I have not yet made a thermo-electric microphone to speak well, partly because iron and German silver do not give a very loud effect, and bismuth is difficult to get in a workable shape; but I hope soon to try the effect of bismuth gauze resting upon iron gauze, and artificially heated.

The recent experiments of Mr. Shelford Bidwell, communicated to the Royal Society on January 18th, 1883, appear to me to bear out the theory that the microphone acts mainly through the air discharge between the points. Thus he finds that the resistance of the contacts varies greatly with the strength of current when the pressure is small, and very slightly when it is great—that is to say, when the film of air is squeezed out. On reducing the pressure (with carbon at least), the resistance of the contact increases. Again, the resistance of the contact diminishes with increased current, an effect also noticed in the voltaic arc. Further, Mr. Bidwell finds that "when the strength of current exceeds a certain limit the resistance is greatly and permanently increased; the greater the pressure the higher will be such limit." This unexpected effect is difficult to explain on any theory but that of the "air discharge." May it not be due to a counter-electro-motive force generated by the discharge. M. Edlund finds that a resistance is set up at the electrodes by a discharge in rarefied air; and the effect observed by Mr. Bidwell may be a similar one.

In his paper to the Royal Society, Mr. Bidwell gives reasons for the superiority of carbon over metals in the microphone. He says: "The mere fact that a current causes delicately adjusted metal contacts to adhere to each other is sufficient to account for the superior efficiency of carbon. A metal microphone might, indeed, be used to transmit the pitch of a sound, provided that its vibrations were sufficiently powerful to cause actual separation of the points of contact. The fundamental tone might in this way be conveyed; but it is clear that the minute superimposed vibrations, due to the upper partials, upon which depends the distinctive character of a particular sound, would be very imperfectly represented, if not entirely lost."

I can only say that we have not found the adhesion of contacts in these metal transmitters troublesome, although iron, according to Mr. Stroh's experiments, has a very high adhesive power. If the battery power is not too great, and the pressure on the contact is not excessive, there will be no sticking of the points.

On the question whether a metal microphone can convey the human voice, I will leave the transmitters to speak for themselves; but I may say that shrill or high-pitched voices, like those of women and children, seem to come out best. This appears to be due to the fact that the discharging distance in the case of metals is very small, and the contacts should not swing too far apart. In a female voice there are twice the number of vibrations per second as compared with the male voice, and the metal points must vibrate in very close proximity when a woman speaks to the instrument. The same peculiarity is, I believe, observed with carbon microphones, and any advantage carbon may have in this respect will lie, I think, in the greater width of arc or discharging distance which it can sustain by reason of the cloud of carbon particles carried by the current between the contacts. I may add that the conducting fumes in the heated metal microphones evidently enable these to sustain a longer arc than if they were cool.

So marked is this difference between male and female voices, that on some of the forms of transmitter I have shown it is possible to hear a female voice with singular clearness when a male voice cannot be understood without altering the adjustment. But, of course, a transmitter purely for female voices would not be very practicable, at least in our day. It would, indeed, be possible to have two transmitters, one for male and the other for female voices, connected at each end of the telephone line, so that a man might speak into one, and a woman into the other. But there would still remain the insurmountable difficulty of providing for the anomalous case of a youth whose voice was in the act of breaking. It would be too much, gentlemen, to ask him to fly from one instrument to another when his voice took a turn in one direction or the other, even if he knew when the anomaly was coming on. Fortunately, however, the gauze transmitter is available for all kinds of voices when properly adjusted; and the same may also be said for the other forms. For male voices more pressure is required on the contacts; but when once an instrument is adjusted for a middle voice, it will convey others of a higher and lower pitch. Thus a transmitter set to an alto voice will also serve for bass and treble speakers.

The **President**: Mr. Munro has given us a very good paper, and I congratulate him on the clear and distinct way in which he

has read the same. There can be no doubt about the importance of the subject, for the time is not far distant when the microphone and telephone will play an important part in our telegraphic systems. A good discussion is sure to follow a paper of this importance, and I shall be glad to hear any remarks which gentlemen present may wish to make.

Professor D. E. HUGHES, F.R.S.: I am not very fond of entering into discussions of any kind, but really I think Mr. Munro has on this occasion brought something rather original in microphones. I have seen so many microphones, which have been always the same thing over and over again, that I could not discuss them. The point brought out to-night by Mr. Munro is one which I tried to point out in my first paper, viz., that by means of metal contacts we could transmit speech just as well as carbon, by employing a number of contacts. I remember showing at Mr. Preece's house, before I brought the paper out (and I think he was very much astonished at it), that if several nails were placed together and spoken to, they would transmit the voice; and that, I think, was the first metal microphone Mr. Preece had seen. I have not heard Mr. Munro's transmitter yet, but I am anxious that he should continue his experiments, because they tend to prove completely the theory I have previously put forward. I tried gauze once, but the experiment, being a hurried one, was not successful. I tried everything, and everything that was a conductor of electricity and had a loose contact spoke; and I have never seen anything having a loose contact that would not speak to a greater or less extent. Some of Mr. Munro's experiments are so exactly similar to those I have made, that I can almost recognise them as my own; but I will give no opinion on the gauze transmitter until I have heard it.

It is unfortunate that the two papers are so much mixed together, because, though I quite agree with Mr. Munro in the construction of his microphone, I cannot well agree to his theory of its action. Every one has a right to his own theory. I have tried very hard to find out the true cause, but, after all the experiments I have made, I am not yet prepared to write the theory of the microphone. It is a very complicated matter. It may be true, as Mr. Munro supposes, that air is between the contact points, but that necessitates taking for granted the fact that air does conduct electricity at a certain distance, and this has not yet been proved. If two plates are insulated from each other by air, we have no proof that electricity will be conducted with sufficient force to work a microphone.

Mr. MUNRO: Within sparking distance.

Professor D. E. HUGHES: Yes. If we lay surfaces on one another, a certain effect takes place; and I have tried to find out if it is surface effect, as Sir William Thomson has supposed. I find that a needle point will speak just as well as the largest surface. In every microphone, if well adjusted, as they ought to be, a peculiar buzzing is heard, as though a transference of matter were going on; and that can be understood, for we know that on the separation of two surfaces through which a current is passing an arc follows. Dr. Warren De la Rue and I have made experiments in that direction, and it can be readily imagined that when two surfaces touch, as in arc lamps, and are then separated, an arc will follow. Then comes the question, Can an arc transmit the voice—is it sufficient? That has been already proved by M. Ader in Paris, who has spoken with a large arc, and also by Professor Blyth; but the difficulty with an arc of some millimètres is in the enormous buzzing that occurs. Dr. De la Rue has made many experiments with myself, as we are both interested in following up this point, and we have made arcs in vacuum tubes, and, when the tubes were spoken to, could distinctly hear the vibration. We have measured extremely small arcs of $\frac{1}{1000}$ of a millimètre, observing by means of a microscope, but the transferrence of matter which always takes place in an arc prevented any exact measurements. The microphonic contact does not obey the laws of ordinary conductors, but it altogether obeys the laws of the electric arc. We have experimented with arcs with a resistance of 5,000 ohms, and arcs with a resistance of 1, 2, or 3 ohms, so that the resistance of an arc may vary to any conceivable extent. I must confess that until a thing is proven I do not like to speak of it; and although we have made a sufficient number of experiments to convince almost any person except myself, I hesitate to bring them forward until the demonstration is to my mind complete. I should like to hear Mr. Preece and other gentlemen who hold different views, and I thank Mr. Munro for having brought this very interesting microphone before us this evening, and hope to hear it after the close of the proceedings.

Mr. W. H. PREECE, F.R.S.: There are so many interests involved in the question brought before us by Mr. Munro, that I hesitate very much to express any opinion on those points that may sooner or later have to be discussed in a court of law. There may be some "chiel amang ye takin' notes," and I should not like at any future day to be served with a subpœna in consequence of any hasty expression that I may have given vent to here. I have not had the opportunity before of reading Mr. Munro's paper, but I have listened to it with a great deal of interest, and I can, like Professor Hughes, compliment him, not only upon having brought before us something new, but on having brought it before us in a very clear, able, and scientific way. I do not agree with all he says, especially on the theoretical part. I have not said much on the theoretical question for two reasons. Firstly, because I hate theory. All young men who first start in the study of electrical science invariably indulge freely in theory. I did so in my earlier days, and I have had to revise my words very often, and, now I am getting a little older, I am rather more chary of discussing, or even entering into theory. I feel that what we have to do now is rather to accumulate facts, and to leave those who have the time, and the leisure, and the authority, to mould these facts into some particular shape. The man of all men who ought to study all these facts, and who ought to string these facts together and produce some theory out of them, is Professor Hughes himself. Whatever views and whatever opinions I may ever have held upon the microphone, I have freely given them to Professor Hughes, and we have had some very noisy and earnest discussions on some of these points.

Now then, as regards the instruments themselves, it appears to me that the whole phenomena embraced by these carbon transmitters, or microphones, or whatever they may be called, was summarised by Professor Stokes when he said that they were "mere phenomena of loose contact." What we have to find is what are these phenomena of loose contact which reproduce sound at a distance. We have seen carbon in various forms devised by Mr. Edison and by Professor Hughes, all acting by loose contact. We have seen to-night metals in several forms produced by Mr. Munro, also acting by loose contact. On this point I would correct Professor Hughes in his remark that the hut of nails was the first and only metal microphone he produced, and I would recall to him that which he made of my gold chain, of a cable pattern, which he succeeded, before several scientific men in his own rooms, in making to speak, and to speak very well.

Two or three theories to account for these phenomena have been mentioned by Mr. Munro. The first is, that the microphone acts from a variation in the resistance of its mass due to pressure; another is, that it acts from an increase or decrease of the surfaces in contact. Professor Hughes leans very much to a theory which he calls the arc theory. I have given vent to my idea on the matter, and I attribute the action at the points of contact to heat. Mr. Munro to-night has brought out a theory in which he tries to convince us that the action is due to silent discharge across a film of air. I will briefly give you my ideas. It is quite true, as Mr. Munro says, that he has not come across my theory. It is not likely that he should have come across it, because I have never yet put it in a paper: I have merely mentioned it rather vaguely in one or two papers—one, particularly, before the British Association last year. Now, I start with this: whether it be a metal conductor, like the one we have seen to-night, or whether it be an ordinary carbon transmitter, we require two conditions. The first is, that there shall be a current flowing; and the second, that in some part of the circuit through which that current flows there shall be a variable resistance, which resistance must vary in proportion to the sonorous vibrations that fall upon it. The first effect that presents itself before us under those conditions is, that whenever a current of electricity flows through resistance there is heat developed, and the temperature of that resistance varies directly with the resistance, and inversely with the mass of the conductor that may form that resistance. The smaller the mass in proportion to the resistance, the greater will be the heat developed there. In fact, the law regulating the development of heat with respect to resistance is perfectly well known; and when we take two balls of metal, or of carbon, or of any of those mysterious materials that are called semi-conductors, we have at the points where they touch resistance, and therefore when a current is flowing, heat. The heat is there and can be seen: it produces light, for it is simply necessary to increase the current strength sufficient to produce light. The heat can be felt, for if a transmitter of the form known as the Hunnings be used, or one of the Edison carbon transmitters, it will be found after a short time that it has become sensibly warmed. Hence there is heat, which is evident to the eye and to the touch. Again, in other microphones, when the current is raised to a certain strength, we have something very like the Trevelyan effect, that is, a humming, spluttering, or buzzing sound. A humming or buzzing can be made by a little adjustment, and one of Professor Hughes' prettiest experiments in his earlier investigations was that in which he made a telephone for a long time maintain a humming or buzzing sound from this effect. Again, we have heat at the point of contact when two metals are brought together, and at a very high temperature, too. Mr. Stroh made that very evident to us when he showed before this Society how he was able to make two pieces of metal stick together, not in the feeble manner Mr. Munro alludes to in his paper, but with a considerable amount of force, so much so that it required a measurable weight to break the "sticktion," or whatever it may be called, which fused the metals together. Again, the resistance at the points of contact of the carbons diminishes with an increment of current, an effect which can be due only to heat. Another point in these microphones, and one which I take to be the real reason why I adhere to my heat theory, is, that in all microphones there is a reversible effect. If a microphone be made to act as a transmitter, it can also be made a receiver. Such an arrangement requires an excessively delicate adjustment, but still Professor Hughes, with his delicate fingers, has succeeded in making a microphone act as a receiver as well as a transmitter. One more point is, that the resistance of carbons and the resistance of metals varies with heat; but the resistance of carbon diminishes, while the resistance of metals increases, and Mr. Munro mentions this fact, but adduces it as one reason against accepting my theory. He says: "In the case of carbon we have a diminution of resistance, and in the case of metal, more especially iron, an increase. Yet, as we have seen, metal points, as well as carbon ones, act as transmitters. The theory, then, does not seem to fit both classes of instruments." But it does; for whether the resistance increases or whether it diminishes is a matter of no consequence whatever—you simply have to produce a variation in the resistance, and such variation may be an increase or a diminution. You want to convert the steady current that is flowing through your microphone into an undulatory one, which will vary in direct ratio to the sonorous vibrations, and whether the microphone contact produce this variation by lowering the resistance or increasing it is a matter of absolute indifference so long as the variation is produced. Now, all these facts together lead me to the conclusion, that whatever may cause the peculiar action of a microphone, it must *ab initio* be a heat effect. The heat is there, and produces all the effects required. It is a very easy conclusion on our part to assume that heat lies at the root of all this. I take my stand from almost the same position as Mr. Munro, and I was led to it a good deal by the action of lightning upon an air film in those plate protectors that act through discharge of lightning, producing absolute conduction between the two plates. In these plate lightning protectors the discharge converts the film of air between them into a conductor as perfect as a metal. This was pointed out in a very clear way by Professor Faraday, who, in his experiments on the Leyden jar, showed that at the moment of discharge the film of air between the two discharge points was absolutely a perfect conductor. And so I say that in the microphone transmitter itself the film of air that exists between the points of contact may become, from the passage of electricity through it generating heat, for the time being a conductor, and its resistance would vary because the thickness of this film varies. We have two points of contact:

these two points are surrounded by a very thin stratum of air, which, by the heat that passes, becomes a conductor. When you speak of throwing the molecules of the microphone into vibration, you increase or diminish the thickness of this conducting air space, and the result is a variation of resistance, and all the effects of the microphone. Mr. Munro, in his theory, has called it a silent discharge across this air space. I do not agree with him on that point, because I regard the silent discharge across an air-space as purely an effect of static electricity; but in the microphone we find that when we go into anything approaching static electricity, when we use high electro-motive forces, the effect ceases. We get the best articulation and greatest effect when we reduce the electro-motive force. Many of the best microphones have been those which worked, not only with an electro-motive force of one volt, but even less than that. Taking all these facts together, I rather come round to the conclusion that there is not very much difference as regards the theory of the microphone in the views held by Professor Hughes, Mr. Munro, and myself. We all agree that the air plays a very important function—that the resistance varies; and all I say is, that the variation is due solely to the action of heat. We are gradually getting to that condition when we may expect a paper from Professor Hughes himself, in which he will take into consideration all that he has heard from Mr. Munro, all that I have tried to put before you, and all that may come afterwards; and we shall, with the aid of his authority, be able to go home, after a discussion in this room, with the full satisfaction that we know a little more about the theory of this beautiful instrument which has almost revolutionised telephony. Mr. Munro in the commencement of his paper, in alluding to the discovery of Professor Hughes, and what has been done in carbon transmitters, stated that "the supremacy of the Edison patent has, in general, paralysed these efforts, and rendered the improvement worthless." I should like to ask whether it has not had the very opposite effect—whether the case of a patent monopolising to a certain extent a great branch of industry does not of itself stimulate no end of minds to try and discover some means of driving a coach-and-four through the very objectionable patent. Although we do object to patents themselves, from a commercial point of view, I think patents have certainly a good productive stimulus in trying to drive active minds to find other means to get out of the trammels that the patent law has put around us.

Mr. A. Stroh: The subject which Mr. Munro has brought before us this evening is an exceedingly interesting one, and one in which I have worked a little at different times. I have also made microphones with metal contacts, but with only partial success, and I came to the conclusion that it was impossible to make a good transmitter with metal contacts. I am therefore very pleased to find that Mr. Munro has succeeded so well with wire gauze, and should like to congratulate him upon his success.

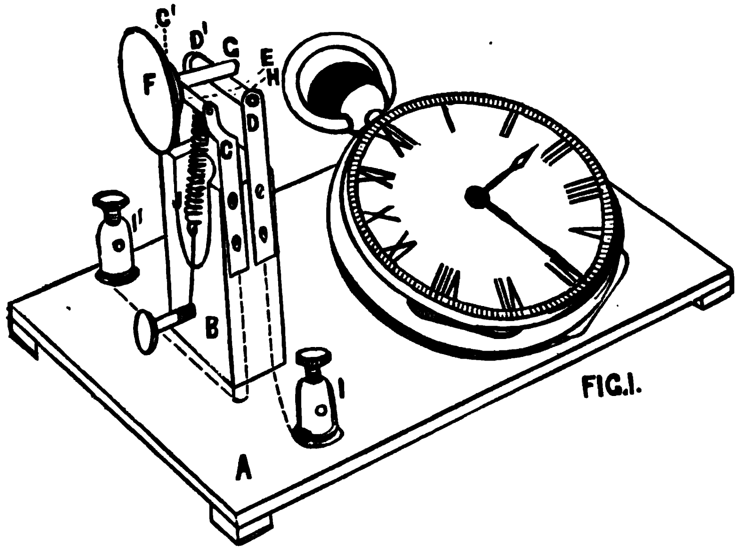

FIG. 1.

I believe that the reason why Mr. Munro has obtained a good result is that the number of contacts between the two pieces of wire gauze in his arrangement are numerous, whereas all who have previously tried to make metal contact microphones have only used single contact, and have either obtained the sticking effect (which I have described in a paper before this Society) alluded to by Mr. Munro, or else have obtained a result which was due to abrupt interruptions in place of mere undulations of the current.

Respecting the theory of the microphone, I may say that I have not formed a very strong opinion as regards this theory, but have made experiments with a view of learning something about it.

One of these experiments appears to me to be an interesting one, and I therefore venture to describe it.

On a thin board, A, Fig. 1, is fixed a little block, B, which serves to hold four brass uprights, C C′ and D D′.

A light spindle, E, provided with a pivot or knife edge at each end, rests on the uprights, C and C′. Attached to the spindle, E, on one side is an exceedingly light concave reflector, F, while on the other side, and in good electrical contact with this spindle, is a thin rod of carbon, G. The latter rests at right angles on another rod of carbon, H, which is firmly fixed in the two brass uprights, D and D′.

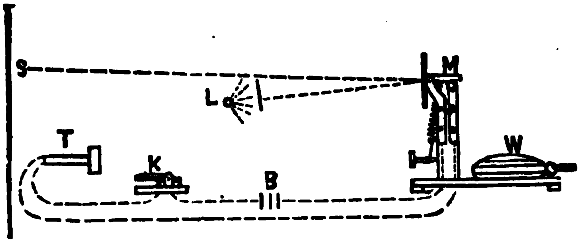

FIG.2.

Terminals I and I′ communicate each with a pair of the brass uprights. A little spiral spring, the tension of which can be regulated by a small thumb-screw, serves to keep the two carbons in microphonic contact.

A loud ticking watch or other source of sound is placed on the board, A.

The general arrangement of the experiment is shown in Fig. 2. M is the microphone just described. A lime light, L, is placed behind a little screen with a small round hole in it, across which is stretched a fine wire, so that the image of the latter is thrown by the reflector of the microphone on a wall or screen at S, where a graduated scale may be fixed, and where by means of the image of the fine wire any deflection can be easily read.

A downward movement of the reflected spot of light on the screen, S, indicates a separation of the carbons at the point of contact, while an upward movement would be due to an approach of the carbons towards each other.

A telephone, T, a make-and-break key, K, a battery, B, and the microphone, M, are joined in circuit. The adjustment of the latter is now made so that the ticking of the watch is heard as loud as possible in the telephone, T, while the observer has one hand on the key, K, and is also watching the spot of light at S.

At the instant when the current is broken at K, the spot of light moves upwards, not sluggishly, but suddenly and sharply, the sound in the telephone of course ceasing at the same moment.

On making contact again, the spot of light quite as suddenly returns to its former position, while the ticking is heard again.

The distance through which the spot of light moves is very small, but the action is so decided and unfailing that there is no difficulty in observing it.

It happens sometimes that microphones are uncertain in their action and will suddenly fail to work; and, in observing the spot of light during some such failures, it was found that it jumped upwards at the moment when the telephone became silent, and returned as soon as the microphone was brought into action again by a tap on the table.

It was *only* during the clear working of the telephone that the spot of light remained steadily deflected downwards, which seems to indicate, that during the time when the carbons are really in what is called microphonic contract they are not in contact at all, or, at all events, that there is a repellant action at the points of contact.

With a single element of a small bichromate battery in the circuit, the deflection was hardly perceptible, but with three elements the spot of light moved through one millimètre between making and breaking.

When a greater number of elements was used the deflection was also greater, but there was much hissing, and the spot of light was not steady.

With three elements the experiment succeeded best. With this number the ticking of the watch could be heard loudly and clearly, without any hissing.

In order to arrive at an estimate of the distance which appeared to exist between the two carbons at point of contact during their action as a microphone, it may be mentioned that the distance between the screen, S, and the reflector was six mètres, and that between the fulcrum and the point of contact of the microphone six millimètres, so that the former distance was 1,000 times greater than the length of lever in the microphone. This figure has to be doubled on account of a reflected beam of light moving through double the angle than the reflector itself.

The total deflection on the screen, S, being one millimètre, the distance to which the upper carbon was repelled during microphonic action under the circumstances described was therefore $\frac{1}{2000}$ part of a millimètre.

I may add that by this experiment I expected also to find, by watching the spot of light on the screen, indications corresponding with each sound or tick of the watch. In this, however, I was disappointed, as the spot of light, after having once moved to the position above described, remained there without any apparent fluctuations, as long as the watch could be heard clearly in the telephone.

Professor W. E. AYRTON, F.R.S.: Mr. Munro commenced his paper by objecting to Edison's expression of a "tension regulator," but I am not sure that this expression, or rather an "electro-motive force regulator," is not quite correct. We have heard to-night, and on many previous occasions, a good deal about the resistance of the contacts in a microphone. I venture to think that we know nothing whatever about resistance proper—in fact, that the word resistance is incorrectly applied to anything like a loose contact. Our whole idea of anything having resistance means that there is something which is constant for any current passing through it. When the resistance of a piece of copper wire is spoken of, it is meant that there is an opposition, or whatever it may be called, which is constant for any current passing through it, assuming, of course, that the body is kept at a constant temperature; and that is exactly what is not the case in a microphone, or in an arc lamp, or in any case where there are two bodies in loose contact. No opposition exists which is independent of the current passing, even if the temperature be kept constant. We have no right therefore to use the word resistance at all in speaking of the microphone or of an arc lamp. What can be said is, that in order to make a certain current pass between two points which do not form a portion of the same homogeneous material, a certain electro-motive force is necessary, and that that electro-motive force depends upon the distance separating the bodies, and also probably on the material of which the bodies are formed. A good deal has been said this evening, and also on previous occasions, on the particles touching. I venture to think that the particles of a microphone never touch, and that is probably the reason why Mr. Munro has found no difficulty whatever in the sticking. In the microphone, as in an ordinary arc lamp, the particles do not touch. What I think is the correct idea of the action of the microphone I first got when making some experiments in 1881 in connection with the electric railway designed by Professor Perry and myself, where we had certain contact boxes. I was very anxious to see whether in the contact boxes we could use paraffin oil. You will all remember Mr. David Brook's plan of using copper conducting wires covered with jute or hemp, and placed in an iron pipe, which was then filled up with paraffin; and I wanted to see if we could extend his plan for our purposes, or, in other words, whether if two metallic bodies were brought together with oil between and all round them there would be any resistance. I found that when the distance between the two bodies was anything measurable, the resistance was practically infinite, whereas when they pressed together, even with a comparatively small force, the resistance was low in spite of the paraffin oil. That means, that when the distance is less than a certain amount, a very small electro-motive force will allow the current to pass. If the distance be greater a very large electro-motive force is required. To maintain a current between two points at a certain distance apart requires a continuous electro-motive force simply depending on the distance. In the ordinary microphone there are two points at a certain distance apart, and the electro-motive force required to maintain a current between those points diminishes or increases, simply because the distance varies. The fact that a microphone can be made a receiver is probably explained by the fact that the transmitter varies the current, which causes the repulsion between the particles to be also varied. There is an attraction depending practically on static attraction, because I believe that, although the particles are exceedingly near to one another, there is a very decided difference of potentials, necessary to enable the current to pass across the space separating them. There is this definite static attraction between the particles, but there is also a much greater electro-dynamic repulsion depending on the amount of current passing; and as the amount of this repulsion is varied by changes in the current strength, the carbon of the receiver oscillates in turn with the sound-waves falling on the transmitter.

As to whether Mr. Preece's heat theory is right or not might be tried somewhat in this way. Mr. Munro mentioned that a metal increases its resistance by temperature, whereas carbon diminishes it. Mr. Preece has rightly pointed out that increase or diminution will answer his purpose equally well, and I would like to ask Mr. Munro whether a substance like German silver will speak. With German silver we are dealing with a substance where the change of resistance is extremely small for changes of temperature. If, for example, a German silver grating speak as well as an iron grating, I think we should have a strong argument against the theory that the action was brought about by or due to change of temperature.

Mr. B. **Warwick**: I should like to be allowed to make a few remarks upon the action of the gauze microphone. Having been engaged with Mr. Munro in a considerable number of experiments, I have been able to examine various forms of microphone transmitters, and it has been clear from the outset that any microphone transmitter having a single pair of contacts was bound of necessity sooner or later to fail, since, as those who operate telegraphic apparatus know full well, one necessity is to frequently clean the platinum contacts. If these contacts are held together by very light springs, they stick and fail to separate after currents have passed. Remembering this, and pursuing the subject, I was led to think of the action which took place actually in the structure of the carbon itself. My observations were prompted by the fact that the discharge which takes place between two pieces of carbon is by no means from a single point, but from a considerable number of points, and the idea was to improve upon that as much as possible. It was quite clear to Mr. Munro and myself that a single contact could not make a microphone in metal, and it was following that line that we were led to adopt gratings, and hence the gauze transmitter before you this evening. This gauze transmitter has 80 meshes to the inch, and it is quite clear that in a circle of an inch diameter there must be a very considerable number of points where two pieces of gauze come together; and it is also quite clear that some of these points rest hard together, and across some of them there is a principal supply of current; also, it is equally clear that there are some points which are so little pressing together that they may be said not to be in contact, and another set of points which are just so far off that no current passes at all. Therefore, the least tremor will rearrange the number of principal contacts, and with the second and third set there is a constant change, during which the current takes up the vibrations, and so we have a practical transmitter. Such is the view I take of the action of the gauze transmitter. I regard the gauze transmitter as the metallic equivalent on a large scale of two pieces of carbon in contact. As to the theory whether the action is due to heat or not, in these experiments I have had opportunity of observing hundreds of cases with the great variety of metals, and found that, if anything like an arc set up, the hissing sound commenced, an effect which is against, if not fatal to any transmitter. I cannot therefore view the action as being due to any arc which causes hissing, buzzing, or heating whatever, but due rather to a silent discharge between the surfaces which are almost touching.

Professor W. GRYLLS ADAMS, F.R.S.: I rise, sir, at your bidding, but I have very little to add to the discussion on this interesting subject. I entirely agree with you, sir, in your remarks with regard to the substance of the paper, and to the clear way in which it has been put before us. The paper is a good one, and has brought out a very satisfactory discussion. The theories put forward are not so very different as they may at first sight appear to be, for whether we regard the current as passing from one point of carbon to another by means of an arc or through the air space between them, the difference is not great. I think that perhaps too much has been made of this arc or air space between the pieces in the carbon or in the gauze transmitter. In the gauze transmitter, where there are many points of contact, the two portions of gauze pressing on one another are probably never out of contact, so that there is no arc or electrical discharge through an air space, but a varying in the strength of current in consequence of the changes in the pressure and in the number of the contacts. The variations in the resistance due to these changes give rise to, and I think explain, the undulatory current better than the theory that there is entire separation between the parts, so causing a spark discharge or a silent discharge through the air space.

More and better contacts diminish the resistance and increase our current; and these changes of current backwards and forwards will not be sudden breaks, because there is no sudden break between the different portions of the metal gauze, but will produce just those undulations which are required to produce the words in the telephone at the other end of the line.

A variation in the number of molecules of carbon or of metal in contact with one another will vary the resistance of the contacts, and so vary the current flowing in the circuit. If there are fewer points of contact it is equivalent to a diminution in the size of the conductor or to an increase in the resistance, which causes an alteration in the strength of current.

Mr. WALTER H. COFFIN: I venture to think that a very brief account of a little instrument I tried last year may be of some interest to Mr. Munro, as confirming his results, and to the Society, as being a very simple means by which most of these experiments can be repeated. I mentioned this form of transmitter to several members on the day we went to Chatham.

The two wires of a circuit, placed parallel to each other, but not in contact, are wound upon a cylindrical surface of about half an inch diameter. On removing the cylinder from the coil, and replacing it by suitable crushed carbon lightly shaken into the cylindrical space (taking precaution against the wires coming into contact), an extremely sensitive and efficient transmitter is formed, which will be recognised as non-tympanic, and a modification of the grain transmitter of Mr. Munro. The next step was to try the effect of metallic particles instead of the carbon grains, and I found that, owing to the elastic nature of the little coil, a single rod of light metal laid inside produced a certain amount of microphonic effect, and iron nails or tacks put into the coil gave very good results. Other metallic substances in various forms furnished effects quite surprising to me, and due, I imagined, to the very great number of contacts; and I think this form of instrument is interesting from the fact that it might possibly be said that the two sides of such a grain transmitter as Mr. Munro's have a tympanic action, or acted as membranes or drumheads. Also it might conceivably be said that in Mr. Munro's very interesting and suggestive gauze microphone the pieces of gauze were tympanic membranes, whereas such a coil form as I mention could hardly be claimed as having a tympanic effect.

The experiments were merely introductory to a research, with the intention of enclosing the microphonic coil in a vacuum to study the effect of rarefaction on different things,—as the alleged arcs, silent discharge, heat effect, and so forth,—and the results seem to indicate some direct effect of aeriform vibration between points very nearly in contact.

The PRESIDENT: In May, 1878, at a special meeting of this Society, Mr. W. H. Preece introduced to our notice those experiments of Professor Hughes which led to the invention of the microphone. In the discussion which followed I stated that I had made a microphone by placing two pieces of iron parallel to each other, with a third piece lying loosely across the top, and I found that if the loose piece was placed near the supported ends of the two parallel pieces the sensitiveness was very slight, but was much increased by moving the loose piece towards the unsupported ends. On changing the pieces of iron for three rat-tail files a better result was obtained, owing probably to the rough surface of the files: this, I think, has some bearing on Professor Adams' remarks that the effect is due to loose contact. I ventured that opinion at the time, and have heard nothing since to lead me to depart from it. I further mentioned that, on placing a lamp (as Mr. Munro did just now) two feet below the rat-tail files, the sound given out by the flame of the lamp was reproduced in the telephone.

Whatever the theory of the microphone may be, there is no doubt that to Professor Hughes belongs the honour of having invented the instrument and given his discovery to the public. I have often felt sorry to see in print that Mr. A or Mr. B has "invented a microphone," but I hope that in future we shall never hear such instruments mentioned except as modifications of Professor Hughes' microphone. Professor Groves gave us a most useful battery, and I never yet heard of any person who, connecting it either for quantity or intensity, would presume to call it his battery. The case is analagous with that of the microphone. Professor Hughes showed us how a simple bar of carbon on two carbon bearings could transmit speech: another man uses two bars of carbon and then calls it his microphone; now that is not fair.

At our next meeting on April 12th, a paper entitled "Microphone Contacts" will be read by Mr. Shelford Bidwell, and I therefore propose that we adjourn the discussion until that evening.

Mr. J. **Munro**: I should just like to say, in answer to Professor Ayrton, that German silver does not give such good results as iron. We have not tried German silver gauze, but two pieces of German silver do not answer so well as two pieces of iron, and therefore we have not brought a German silver instrument before the meeting.

The **President**: I will ask you to accord a vote of thanks to Mr. Munro for his able paper.

This was heartily accorded.

A ballot then took place at which the following were elected:—

Transferred from the class of Associates to that of Members—

Thomas Fletcher.
Charles Streatfeild James.

Elected—

As Foreign Member:

Gregoiré Skrivanow.

As Member:

Captain William Arthur, R.N., C.B.

As Associates:

George Agnew.
F. W. Andersen.
Francis J. Bell.
W. Betts.
James Brand.
Thomas Augustus S. Clack.
A. de B. Creed.
Ernest George Dornbusch.
José D. Husbands
James Meldrum Irvine.
Robert Samuel Lloyd.
Philip Monson.
Joseph Morgan.
William Robinson.
W. Rowbotham.
Alexander Schanschieff.
H. S. Shillito.
Alfred Walter Soward.
Colman Charles Starling.
William Todhunter.
A. F. Toovey.
Colonel the Hon. W. Le Poer Trench, R.E.
Frederick Trimmer.
J. W. Ward.
Thomas Williams.

As Students:

William Meredith.
Herbert Laws Webb.
H. M. Window.

After which the meeting adjourned until Thursday evening, April 12th, 1883.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY DURING FEBRUARY AND MARCH, 1883.

By Alfred J. Frost, *Librarian.*

(*Works marked with an Asterisk * have been purchased.*)

Ampere [André-Marie]. The Story of his Love; being the Journal and Early Correspondence of André-Marie Ampère with his Family Circle during the First Republic, 1793-1804. Edited by Madam C. H. 8vo. 328 pp. *London*, 1873

Bell [Alex. G., Ph.D.]. Upon the Electrical Experiments to determine the location of the bullet in the body of the late President Garfield, and upon a successful form of Induction Balance for the painless detection of metallic masses in the human body. 8vo. 58 pp. [American Association for the Advancement of Science, Montreal Meeting, August, 1882.] *Washington, D.C.*, 1882

Berthoud, Borel, & Co.'s Conductors. 4to. 1 p. [Reprinted from the *Mechanical World*, Dec. 30, 1882.] *London*, 1882

——— Patent Electric Cables. Fo. 1 p. *London.*

——— System of Cables. Casing Telegraph Wires. 4to. 3 pp. [Extract from *Engineering*, Nov. 11, 1881.] *London*, 1881
[Presented by the Electrical Trading Co., Limited.]

Borel. [*Vide* Berthoud, Borel, & Co.]

Boys [C. Vernon]. Description of Boys' Engine-power-meter, with practical directions for its use. 4to. 16 pp. *London.*

——— Patent Electric Meters. 8vo. 7 pp. *London*, 1882
[Presented by the Electrical Trading Co., Limited.]

——— [*Vide* Forbes.]

Brandes [Otto]. [*See* Eger.]

* **Bresch** [Richard]. Der Chemismus, Magnetimus, und Diamagnetismus im Lichte mehrdimensionaler Raumanschauung. 8vo. 146 pp. *Leipzig*, 1882

C. H. [Madam]. [*See* Ampère.]

Canton. [*Vide* Michell and Canton.]

D'Auvergne [Capt.] [*Vide* McCulloch, K.]

Du Moncel [Comte Th.] Elements of Construction for Electro-Magnets. Translated from the French by C. J. Wharton. 8vo. 86 pp. *London*, 1883
[Presented by the Translator.]

Edison [T. A.] Sketches at Thos. A. Edison's Laboratory at Menlo Park, N.J... A Visit to the Inventor of the Phonograph. Illustrated. [The *Daily Graphic*.] *New York*, 1878

Edison [T. A.] La Lumière Edison. Système d'Éclairage Électrique. Transmission de la force motrice à domicile. La presse française et les système Edison. 8vo. 86 pp. *Paris*, 1882
[Presented by S. M. Banker.]

Eger [Gustav], **Brandes** [Otto], and others. Technological Dictionary in the English and German Languages. Edited by Gustav Eger. In Two Parts. 1st Part, English-German, by Otto Brandes. 8vo. 711 pp. *Brunswick*, 1882

* **Electrician** [The]. A Monthly Journal of Electrical Science. 4to. Commenced 1882. *New York*, 1882

—— The Electrician's Directory, with Diary, for 1883. 8vo. *London*, 1883
[Presented by the Publisher.]

Elphinstone [Lord] **and Vincent** [Charles W.] The Elphinstone and Vincent Continuous Current Dynamo-electric Machines. 8vo. 18 pp. *London*, 1883
[Presented by Mr. Theodore Baxter.]

* **Fenwick** [Thomas]. A Theoretical and Practical Treatise on Subterraneous Surveying, and the Magnetic Variation of the Needle. 8vo. 207 pp. Plates. *Newcastle-on-Tyne*, 1804

Forbes [Prof. Geo.] Report to Electrical Trading Company, Limited, on the Electric Meters and other Machines invented by Mr. C. Vernon Boys. 4to. 2 pp. *London*, 1882
[Presented by the Electrical Trading Co., Limited.]

Graff. Die Kosten der elektrischen beleuchtung. La 8vo. 7 pp. Plate. [Separatabdruck aus der *Zeitschrift für Angewandte Elektricitätslehre*, 1880.] 1880

* **Hancock** [Thomas]. Personal Narrative of the Origin and Progress of the Caoutchouc or India-rubber Manufacture in England. 8vo. 283 pp. *London*, 1857

Hauck [W. Ph.] Die Galvanischen Batterien Accumulatoren und Thermosäulen. 8vo. 320 pp. [A. Hartleben's *Elektro-technische Bibliothek*. Band IV.] *Wien, Pest, Leipzig*, 1883
[Presented by A. Hartleben.]

* **Holmes** [A. Bromley]. The Electric Light popularly explained. 3rd Edition. Sm. 8vo. 110 pp. *London*, 1882

Hopkinson [Dr. J.], F.R.S. Residual Charge of the Leyden Jar.—Dielectric Properties of different Glasses. 4to. 28 pp. [*Phil. Trans.* Vol. 167. Part II.] *London*, 1877

—— On the Thermo-elastic Properties of Solids. 8vo. 6 pp. *London.*

—— On the Torsional Strain which remains in a Glass Fibre after release from Twisting Stress. 8vo. 7 pp. [*Proc. Roy. Soc.*, No. 191.] *London*, 1878

—— Electrostatic Capacity of Glass. 4to. 7 pp. [*Phil. Trans.* Part I.] *London*, 1878

—— On Electric Lighting. (First Paper.) 8vo. 28 pp. [*Proc. Inst. Mech. Eng.*, 25th April, 1879.] *London*, 1879

—— On the Stresses caused in an Elastic Solid by Inequalities of Temperature. 8vo. 7 pp. [Extract from *The Messenger of Mathematics*. New Series, No. 95, March.] *London*, 1879

—— On High Electrical Resistances. 8vo. 3 pp. [*Phil. Mag.*, March.] *London*, 1879

Hopkinson [Dr. J.], F.R.S. On Electric Lighting. (Second Paper.) 8vo. 20 pp. [*Proc. Inst. Mech. Eng.*; 23rd April, 1880.] *London*, 1880

—— The Electrostatic Capacity of Glass. [Abstract.] 8vo. 2 pp. [*Proc. Roy. Soc.*, No. 208.] *London*, 1880

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—— On the Refractive Index and Specific Inductive Capacity of Transparent Insulating Media. 8vo. 3 pp. [*Phil. Mag.*, April.] *London*, 1882

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—— Administration Report of the Indian Telegraph Department for 1880-81. Fo. 45 pp. Plates. *Calcutta*, 1881

—— Code for Licensed Telegraph Offices. Part I. General Rules. 8vo. 79 pp. *Calcutta*, 1882

—— Code for Licensed Telegraph Offices. Part II. Technical. 8vo. 28 pp. 23 pp. of Plates. *Calcutta*, 1882
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—— Direzione Generale dei Telegrafi, Guida degli impiegati telegrafici. 1. Parte Amministrativa—Servizio de telegrammi. 8vo. 368 pp. *Roma*, 1882

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[Presented by the Local Hon. Sec. for Italy.]

Julia [D. Enrique]. Anuario de comunicaciones telegráficas para el año de 1883. Redactado por D. Enrique Juliá, jefe de Estacion del cuerpo de Telégrafos y los Officiales del mismo, D. Ildefonso de las Heras y A. Alfonso Márquez Primer Año. 8vo. 98 pp. *Cadis*, 1883
[Presented by the Direct Spanish Telegraph Co., Limited.]

Kabath [N. de.] Accumulateurs électriques a lames Gaufrées. 8vo. 31 pp. [Reprinted from *L'Electricien.*] *Paris*, *n.d.* (1881?)

* **La Electricidad.** Revista general de sus progresos cientificos é industriales Año 1. Núm. 1. [Jan. 1.] Sm. Fo. *Barcelona*, 1883

* **McCulloch** [K.] An Account of the new Improved Sea Compasses, with Reports of their Practical Utility, founded on some years' experience. By Capt. Philip D'Auvergne, F.R.S., and other scientific gentlemen. 8vo. 30 pp. Plates. *London*, 1789

* **Mackintosh** [T. Simmons]. The Electrical Theory of the Universe; or, The Elements of Physical and Moral Philosophy. 8vo. 468 pp. *London*, 1838

* **Michell** [J.] **and Canton** [J.] Traités sur les Aimans artificiels. . . . Traduits de deux ouvrages anglais de J. Michell and J. Canton, par le P. Rivoire de la C. de J. Sm. 8vo. 160 pp. Plates. *Paris*, 1752

* **Morgan** [Chas. E.] Electro-Physiology and Therapeutics. . . . 8vo. 714 pp. *New York*, 1868

Moulton [J. Fletcher]. See Spottiswoode and Moulton.

Munich International Electrical Exhibition. Certificate of Thanks to the Society of Telegraph Engineers and of Electricians. Fol. 4 pp. *Munich*, 1882

Philippe. La télégraphie aux armées. 8vo. 63 pp. *Paris*, 1883

* **Plum** [Wm. R., LL.B.] The Military Telegraph during the Civil War in the United States; with an exposition of ancient and modern means of communication, and of the Federal and Confederate Cipher Systems. Also a running account of the War between the States. 2 Vols. 8vo. 377 and 390 pp. *Chicago*, 1882

Raynaud [Jules]. Exposé Sommaire de la mesure électrique en unités absolues. 8vo. 52 pp. *Paris*, 1882

* **Serpieri** [A.] Il potenziale elettrico nell'insegnamento elementare della elettrostatica. La moderno teoria dei fenomeni elettrici trattata in modo elementare per i licei e gli Institute Italiani. La 8vo. 171 pp. *Milano*, 1882

* **Sidney** [Edwin]. Electricity: its Phenomena, Laws, and Results. 16mo. 184 pp. *London*, 1843

Spottiswoode [Wm.] On Multiple Contact of Surfaces. 4to. 29 pp. [*Phil. Trans.* Vol. 166. Part I.] *London*, 1875

Spottiswoode [Wm.] **and Moulton** [J. Fletcher]. On the Sensitive State of Electrical Discharges through Rarefied Gases. 4to. 6 Plates. 66 pp. [*Phil. Trans.* Part I., 1879.] *London*, 1879

—— On the Sensitive State of Vacuum Discharges. Part II. 4to. 5 Plates. 92 pp. [*Phil. Trans.* Part II., 1880.] *London*, 1880

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* **Thompson** [Prof. Silvanus]. Cantor Lectures on Dynamo-electric Machinery. La. 8vo. 54 pp. [Reprinted from the *Journal of the Society of Arts.*] *London*, 1883

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Tommasi [Dr. Donato]. Recueil de quelques mémoires publiés pendant l'année 1882 dans le comos-les-mondes. La. 8vo. 18 pp. *Saint Denis*, 1883

Urbanitzky [Dr. Alfred von]. Das elektrische licht und die hierzuangewendeten Lampen, Kohlen, und Beleuchtungskörper. 8vo. 223 pp. [A. Hartleben's *Elektro-technische Bibliothek.* Band III.] *Wien, Pest, Leipsig*, 1883 [Presented by A. Hartleben.]

—— Die elektrischen beleuchtungs. Anlagen mit besonderer Berücksichtigung ihrer praktischen Ausführung. 8vo. 240 pp. [A. Hartleben's *Elektro-technische Bibliothek.* Band XI.] *Wien, Pest, Leipsig*, 1883 [Presented by A. Hartleben.]

Vincent [Chas. W.] [*Vide* Elphinstone and Vincent.]

Wharton [C. J.] [*See* Du Moncel.]

* **Wiedemann** [Gustav]. Die lehre von der elektricität—zugleich als dritte völlig umgearbeitete auflage der lehre vom galvanismus und elektromagnetismus. Zweiter Band. La. 8vo. 1002 pp. *Braunschweig*, 1883

The following is a List of the Periodicals, Transactions, and other Serials in progress, which are received in the Library of the Society.

Those which have an asterisk (*) prefixed are purchased—the greater part of the remainder being received in exchange for the Journal of the Society.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
* Electrician.
English Mechanic.
Engineer.
Engineering.
Greenwich Magnetical and Meteorological Observations.
Incorporated Law Society Calendar.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
Journal of Science.
Meteorological Society, Proceedings.
Nature.
Patents' Journal, Commissioners of.
* Philosophical Magazine.
Physical Society, Proceedings of.
Royal Engineers' Institute, Proceedings of.
Royal Institution, Proceedings of.
Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Engineers, Proceedings.
Telegraphic Journal and Electrical Review.
University College Calendar.

AMERICAN.

American Electrical Society, Proceedings.
Electrical Review.
* Electrician.
Johns Hopkins University Circular.
Journal of the Telegraph.
Scientific American.
Smithsonian Institution Reports.
United States Patent Office, Official Gazette of.

FRENCH.

Annales de l'Électricité.
Annales Télégraphiques.
* Bulletin de la Compagnie Internationale des Téléphones.
Journal de Physique.

Journal Télégraphique.
* La Lumière Electrique.
L'Électricité.
L'Électricien.
Revue Scientifique.
Société des Ingénieurs Civils, Mémoires.
Société Française de Physique, Séances de la.
Société Scientifique Industrielle de Marseille, Bulletin de la.

GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Electro Techniker.
Elektrotechnische Zeitschrift.
Repertorium für Experimental—Physik für physikalische—Tecknik.
Verhandlungen des Vereins zur Beförderung des Gewerbfleisses.
Zeitschrift für Angewandte Electricitätslehre.
Zeitschrift für Instrumentenkunde.

ITALIAN.

Government Telegraph Department, Annual Report.
Il Telegrafista.
Natura.

RUSSIAN.

Government Telegraph Department, Annual Report.

SPANISH.

* La Electricidad.
Revista Topografica y Estadistica.
Revista de Telégrafos.

ORIGINAL COMMUNICATIONS.

A DIFFICULTY IN ELECTRIC DISTRIBUTIONS.

By W. E. AYRTON and JOHN PERRY.

We have asserted several times that, although the methods of M. Deprez and ourselves enable us to keep two points in a system of distribution at a constant difference of potential, whatever may be the consumption of electric energy throughout the system which proceeds from these points, there is no published method by means of which *every* point in the system may be maintained at a constant potential. Our attention has been drawn to the fact, that in the absence of exact proof there is still a doubt on the subject in the minds of many electricians, and we now propose to give this proof.

In Fig. I. let A and a be the points maintained at a constant difference of potential, V; let the supply cable be A M N P, and the return cable $a\ m\ n\ p$, and let M and m be the terminals of a consumer, N and n being the terminals of another consumer. Suppose that from M to m there is a current, c, passing, and from N to n a current, c'. Let resistance A M be called R, and resistance $a\ m$ be called r. Let x be the difference of potentials between M and m. Then $x = V - (c + c')(R + r)$, and obviously, if the second consumer stops his current, x becomes $V - c(R + r)$: that is, the one consumer's potentials are altered by another consumer stopping his current. And it is not merely the consumer who happens to be near the central station who is so affected, for it will be found in the same way that every consumer is affected by the turning on or off of any other consumer's supply so long as their mains proceed from and return to the same terminals.

It is sometimes thought that the arrangement shown in Fig. II., known as that patented by Edison, will prevent this interference. As before, let the resistance A M be called R, $m\ a$ be called r, A N be called R′, and $n\ a$ be called r'. Then if, as before, current

c' passes from N to n, and c from M to m, the difference of potentials between A and a being V, and x the difference of potentials between M and m, and x' the difference of potentials between N and n, then

$$x = \mathrm{V} - (c + c')(\mathrm{R} + r') - c(r - r'),$$

and

$$x' = \mathrm{V} - (c + c')(\mathrm{R} + r') - c'(\mathrm{R}' - r).$$

Hence, in the first place, for the two consumers to have the same differences of potential they must use supplies of electricity bearing the same proportion to one another that the resistance from M to N does to that from m to n. Hence the advantage usually claimed, and claimed, for example, on page 120 in the discussion on Mr. Shoolbred's paper, that all the consumers have always the same potential, is quite unreal. We have now, however, to consider the more important advantage which has also been claimed, namely, that the potential of the consumer M m remains constant. Altering the first expression given above, we find

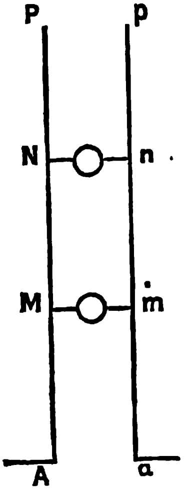


FIG. I.

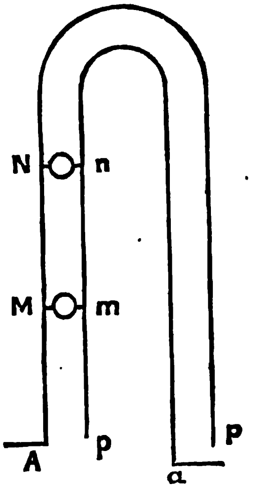


FIG. II.

$$x = \mathrm{V} - c(\mathrm{R} + r) - c'(\mathrm{R} + r'),$$

so that there is an obvious dependence of x on the value of c'. In fact, the potential of any consumer is altered by other consumers taking or stopping their supply, whether the distribution system of Fig. I. or Fig. II. is employed.

EARTH CURRENTS IN INDIA.

By E. O. Walker, Member.

I should like to add a few remarks to my contributions on "Earth Currents," which appeared in the last issue of the Journal. Further observations have corroborated those already made known. The currents now, in March, are mainly of the same strength, and have the same times of zero and maxima, and flow in the same directions for the same periods as in August last. But it has struck me that, for the sake of comparison, the difference of potential between two stations should be given in records, and not the strength of the current. I think it would be found that, if this were done, the variations in results now remarkable in different parts of the globe would not exist. The following circumstances modify the strength of the current:—

(1.) Resistance of galvanometer.
(2.) Gauge of line wire.
(3.) Resistance of instruments of intermediate offices.

And to say that a current of ·5 milli-ampère traversed the line from London to Cardiff, and a current of ·2 milli-ampère, a line in similar geographical position elsewhere at the same time, does not convey a clear idea of the circumstances.

If there be a general agreement that the difference of potential shall be recorded, we should know in any series of observations between two stations the direction in which a current flowed, and could, if desirable, mark out the strength of current from local data.

The register should state—

Cardiff-London line, No. 6; gauge, No. 5½; E. to W.; length, 200 miles; resistance of galvanometer, 1,500; tested resistance of line, 2,100; resistance of intermediate instruments, 500; time, 9.30; date, 24/3/83; temperature, $\frac{50^\circ}{55^\circ}$; weather, $\frac{\text{cloudy}}{\text{rain}}$; potential at London, 2·1 volts.

This would show that the current at 9.30 a.m. on this day flowed from London to Cardiff; later in the afternoon further entries would show perhaps that the difference of potential was now in favour of Cardiff, and these would again show us that now the direction of the current had changed.

From existing observations it seems very probable that if a wire completely circled the earth we should get no current at all; that if we had four lines each 6,000 miles long, one in each quadrant of the globe, in the same latitude, we should have a most perfect system for observations; but as it is impossible to obtain this at present, the next best thing is to get long lengths of lines joined together, but not overlapping each other's confines, and on such lines in all parts of the globe to institute simultaneous observations by Greenwich time. The most important facts to notice are the times of occurrence of maxima and minima (which are really zeros), and the differences of potential creating the maxima, with temperature, weather, and barometrical readings. If the Council of the Society will appoint a date for commencing observations and the fixed period, and will draw up the form of register to be observed, I for my part shall be glad to assist, and I have no doubt that many in other parts of the world will readily come forward.

I should mention that I find it is a matter of indifference, on a long line, whether iron or copper earth plates are used with these observations. On a very short line galvanic action is apparent; not so on a line of comparatively high resistance.

It is better, of course, to have both plates of the same metal.

INCREASE OF QUANTITY IN A SINGLE LIQUID CELL BY AGITATION OF THE SOLUTION.

By E. O. Walker, Member.

Some experiments were made to ascertain if there were any increase in current by causing the solution in a single fluid element to be boiled. An empty one pound biscuit tin was taken, set length upwards, some coal cinders placed at the bottom, some loose stones on the cinders to keep them from floating; then the tin half filled with a solution of 2 oz. of sal ammoniac in water, and the cell completed by suspending a piece of zinc in the liquid just below the surface. The edge of the tin box formed one terminal, and the wire connected to the zinc another. A lamp was lighted under the tin box and the effect observed on a tangent galvanometer, though the thick coil (R=0·75 ohm) alone, and through the same, with 20·4 ohms additional resistance.

	Thick.	Thick + 20.	R.	C.
Readings at first	53°	18°	5·84	·076
Ten minutes	55°	18°	5·24	·084
"	62°	19°	3·82	·109
"	71°	20°	2·17	·170
"	78°	24°	1·38	·235
Boiling and agitated ...	80°	23°	0·90	·364
Cooling again...	75°	19°	1·33	·240
" " ...	69°	15°	1·50	·222
" " ...	65°	13°	1·72	·202
" " ...	60°	11°	1·82	·194
" " ...	55°	10°	2·11	·175

Now it appeared as if the boiling had caused this immense increase of quantity, but on recharging the cell with fresh mixture the readings were 75° to 22°, so that the act of boiling first dissolved the salts perfectly, but the agitation and heat actually only raised the deflection from 75° to 80°. This, however, is a considerable increase, since the tangents of these angles are 3·73 and 5·67 respectively; so that in the production of electricity from a battery for lighting purpose, etc., it would be of advantage to keep the liquid agitated by boiling or by blowing air through it. The electro-motive force of this cell is about 0·5 volt.; without the cinders, the deflection falls from 75° to 60°, and rises again to 75° on replacing the cinders; in an earthenware jar instead of the biscuit tin, 70° as against 75°, but an electro-motive force in this case of 1 volt: thus, the tin helps to reduce the resistance of the cell, but the local action lowers the electro-motive force. Such a cell naturally rapidly exhausts when on short circuit. With the tangent galvanometer used in this experiment a Minotti cell of 19·5 ohms resistance will give deflections of 52° and 33°.

BELGAUM, *5th March*, 1883.

ABSTRACTS.

J. STEFAN—THE ACTION OF IRON AS A MAGNETIC SCREEN.

(*Annalen der Physik und Chemie, B.* 17, *H.* 5, *No.* 13, 1882, *pp.* 928-955.)

[It may be well at the outset to remark that the term "screen" does not seem very well chosen, since by "screen" we generally understand something interposed on the straight line joining two points; but in the present investigation the term "screen" is applied to a mass of iron placed *anywhere* in the field of force of a magnet.]

The author refers to the observation of Poisson, in his theoretical discussion of magnetism, that when a hollow iron sphere is subjected to the action of magnetic lines of force, the effects produced in the hollow interior are much less than those outside the sphere, the mass of iron acting as a magnetic screen; and he then goes on to give an account of some recent experiments on the subject made by himself.

I. Deflection Experiments.

The magnet to be deflected was the ring magnet of one of Edelmann's galvanometers. The deflecting magnet was placed in a line passing through the centre of the ring magnet of the galvanometer, and at right angles to the magnetic meridian. At a distance of 31 cm. a cylindrical bar magnet, 5 cm. long and 1 cm. in diameter, produced a deflection of 116 divisions of the scale. When a short iron tube, 5 cm. long, 1 cm. thick, and 11 cm. internal diameter, was placed concentrically over the bar magnet, so as to form an envelope to it, the deflection was reduced to 22 divisions, or to one-fifth of its original value. An iron tube, 10 cm. long, but otherwise of the same dimensions, and which therefore projected beyond each end of the bar magnet, reduced the deflection to 12 divisions.

A bar magnet, 10 cm. long and 1 cm. in diameter, produced, at a distance of 34·3 cm. from the galvanometer, a deflection of 495 divisions, and at 41·4 cm., 281 divisions. By slipping the 5-cm.-long tube over the magnet, which in this case projected beyond its envelope, the above deflections were reduced to 70 and 41 divisions respectively.

An iron tube also diminishes the effect of the deflecting magnet if it be placed above or below the latter, but in the same plane with it, instead of enveloping it on all sides, as is shown in the following table, in which *h* is the vertical distance between the axis of the bar magnet and the axis of the iron tube. I. refers to the magnet 5 cm. long, and II. to the 10-cm. bar, while S gives the divisions of deflection.

	h = 0	2·5	5	7·5	10	12·5	∞
I.	S = 22	35	65	90	102	110	116
II.	S = 70	127	184	283	436	463	495

The action of the magnet is also diminished if a flat iron plate be placed either above or below it. A round iron disc, 1·2 cm. thick and 18 cm. diameter, was placed in a horizontal position at varying distances (h) above the 10-cm. magnet, and the following deflections were noted:—

h = 2·5	5	7·5	∞
S = 276	388	440	495

If the disc is placed by the side of the bar magnet, so that its horizonal diameter is parallel to the axis of the magnet, a similar effect is produced; but if the plate is placed either in front of or behind the deflecting magnet, a small increase of effect is observed instead of a decrease.

II. Oscillation Experiments.

A small cylindrical magnet, 3 cm. long and 0·4 cm. in diameter, was suspended by a fibre, and the time of oscillation was 5·9 seconds; with a bar of brass in place of the magnet the time of oscillation was 34·4 seconds. If a hollow iron cylinder, 5 cm. high, 11 cm. internal diameter, and 1·5 cm. thick, was now placed surrounding the magnet in such a position that they had a common axis, and the centre of the iron cylinder coincided with the centre of the magnet, then the time of oscillation was considerably increased. Not only did this increase take place when the centre coincided, but also when the magnet was displaced so as to be above the axis of the hollow iron cylinder, as is shown in the following table, in which h is the height of the centre of the magnet above the axis of the cylinder, and τ the corresponding time of oscillation. $T/_{T'}$ is the ratio between the horizontal component of the earth's magnetism and the intensity of the magnetic field at the varying points of the magnet. These figures show how much the effect of the earth's magnetism has been diminished by the influence of the iron cylinder:—

h = 0	1·25	2·5	5	7·5	10
τ = 13·6	12·8	11·0	8·4	7·1	6·6
$T/_{T'}$ = 6·10	5·30	3·76	2·09	1·47	1·26

A second series of observations with a cylinder 5 cm. high, 11 cm. internal diameter, and 1 cm. thick, gave the following values:—

h = 0	2·5	5	7·5
τ = 12·4	9·7	7·1	6·0
$T/_{T'}$ = 4·91	3·38	1·93	1·4

A third cylinder 10 cm. high, 11 cm. internal diameter, and 1 cm. thick, was then tried, with the following results:—

h = 0	2·5	5	7·5	10
τ = 17·2	17·0	10·4	7·2	6·0
$T/_{T'}$ = 10·31	7·84	3 65	1·96	1·4

III. INDUCTION EXPERIMENTS.

A bobbin is placed with its axis perpendicular to the plane of the magnetic meridian, and, whilst always kept in the same relative position, it is gradually inserted into a hollow cylinder of soft iron, on which a very feeble induced current is produced. The effect is barely noticed unless a very powerful magnetic field is used, and the author therefore made use of an electro-magnet constructed on Ruhmkorff's pattern. If a bobbin is brought up from a distance into the magnetic field, a current is induced; if then a cylinder of soft iron is brought up so as to surround the bobbin, an induced current equal in intensity is set up, but in the contrary sense to the former one.

In the above experiments the plane of the convolutions of the bobbin was perpendicular to the lines of force. A variation in the experiment may be made by placing the plane of the convolutions parallel to the lines of force, and passing an iron disc through them, then the lines of force pass symmetrically through the disc. If the disc is covered on one side with an annular piece of soft iron, a current is produced in one or the other direction, according as the convolutions are nearer to the one or other edge of the disc.

Very significant results are obtained by winding the convolutions of wire on the wall of the cylinder longitudinally parallel to the axis of the cylinder, as is done in the Pacinotti ring and in the Gramme machines. If the bobbin thus constructed be placed in the same magnetic field as before, and the coils are made to rotate around the axis of the iron cylinder, a current is induced. A current is also produced if the coils and the cylinder are rotated together; but no current results if the coils are kept still and the cylinder is rotated.

The experiment can be modified by making the coils and the cylinder rotate around different axes, or by giving one a motion of translation with respect to the other. In the latter case, if a translatory motion is imparted to both coils and cylinder in the same direction, there is no current; but, in order to produce a current, there must be a relative displacement.

This last observation is curious: in fact, if it is closely examined, it is seen that the portion of the wires which are inside the cylinder move in a magnetic field which is weaker than that in which are the exterior wires; and it would therefore seem that a current ought to be set up. But on moving the cylinder in the magnetic field, the lines of force, which may be looked upon as physical lines, move from the front part of the tube towards the back part, in such manner that the wires inside cut as many lines of force as the wires outside, and there is thus no current.

To make more sure of this result, the author tried the following experiment:—Two binding screws were fastened at the corners of a table and connected with a galvanometer. In the binding screws, two stout copper wires, bent in form of an arch, were fastened; and the ends of these two arches were united by a copper wire stretched along a lath. The line joining the binding screws, as well as the lath, were placed at right angles to the magnetic meridian, so that the bent wires formed two vertical planes parallel to the latter. If the latter was moved in the direction of the magnetic meridian, the vertical component of the earth's magnetism induced a current which could be measured on the galvanometer. If the wire on the lath was surrounded by an iron tube, and the experiment repeated, the current produced had the same value as before.

From the equality of the two currents, it follows that the progressive motion of a closed conductor in a homogeneous magnetic field is productive of no induced current if a part of the conductor is surrounded by a tube of iron. Further, the author shows that a current which flows through an iron tube in the direction of its axis, imparts a progressive motion to the tube, which is at right angles to the axis and the magnetising force of the earth.

G. GUGLIELMO—THE USE OF AN ELECTROMETER IN THE MEASUREMENT OF LIQUID RESISTANCE.

(*Beiblätter*, *B.* 6, *St.* 10, 1882, *p.* 803.)

The author finds that for the determination of resistances Wheatstone's method is much preferable to Mance's; and he makes use of an electrometer in connection with the bridge method. The current of the battery is only momentarily closed, and immediately the electrometer is disconnected from the bridge by a suitable key. The time between the closing of the battery circuit and the cutting out of the electrometer is extremely short, and allows the polarisation no time to act on the electrometer.

For the determination of the E.M.F. of polarisation, Guglielmo connects the cell in the branch, C D, of the bridge, A B C D A, as well as the polarisation apparatus; makes the branch $A B = B C = r$; connects D to earth, B to the electrometer, and makes the resistance of A D and C D equal. Then the difference of potential shown on the electrometer $\Delta = \frac{1}{2} (E - e)$, where E is E.M.F. of the cell, and e that of polarisation. If, then, E is known, e can be calculated. If, instead of putting the junction D to earth, a point, D′, between the cell and the polarisation apparatus be joined to the earth wire, and the branches are arranged so that $A B = B C$ and $A D' = D' C$, the difference of potential on the electrometer $\Delta' = \frac{1}{2} (E + e)$, so that now both E and e can be determined.

P. SAMUEL—A NEW METHOD FOR MEASURING THE INTERNAL RESISTANCE OF BATTERIES.

(*Beiblätter*, *B.* 6, *St.* 11, 1882, *p.* 885.)

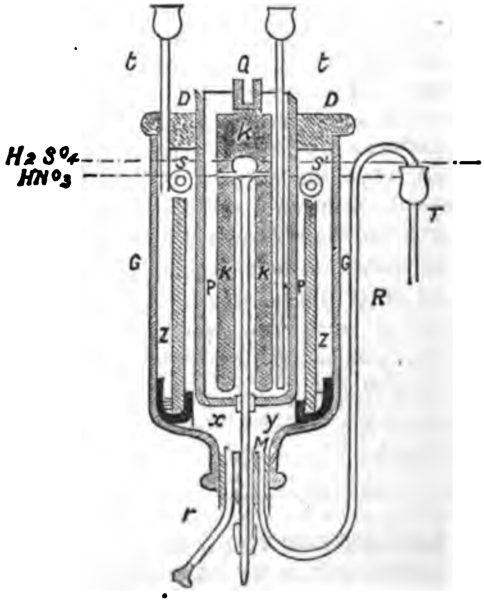
The author proposes to conduct the secondary current of an induction coil through the two outer equal and co-axial coils of a Hughes sonometer, and to connect the third movable coil with a telephone, the movable coil being placed in such a position that the currents induced in it by the outside fixed coils neutralise each other, and no sound is heard in the telephone. The cell to be experimented on is then joined up in the circuit of one of the two fixed coils, while by means of a rheostat a varying resistance is introduced into the circuit of the other coil, until the telephone again gives no sound. Then the resistance of the battery is equal to the resistance so introduced.

The secondary current of an induction coil can also be conducted through an ordinary Wheatstone bridge arrangement, a telephone being joined up in place of the usual galvanometer. If the battery and the variable resistance are in two consecutive branches of the bridge, and the latter is altered until there is no sound in the telephone, it will be equal to the battery resistance. This method is, however, less exact than when the sonometer is used.

Dr. E. OBACH—A NEW FORM OF BATTERY FOR PRODUCING A POWERFUL AND CONSTANT CURRENT OF LONG DURATION.

(*Carl's Repertorium, B.* 18, *H.* 11, *pp.* 633–650.)

In carrying out a series of experiments with his movable ring tangent galvanometer, the author was in want of a battery which should give a considerable current for a length of time, and yet be quite constant, and he accordingly devised the form of cell which he describes.



It is a zinc-carbon element, the exciting fluids being as usual dilute sulphuric and strong nitric acids, which, owing to the arrangement of the cell, are continually renewed. The internal resistance of one of these cells is on the average 0·07 ohm and the E.M.F. 2·09 volts; it can therefore produce, when short-circuited, a current of nearly 30 ampères.

The accompanying figure and description will render the construction clear. G G is a wide-mouthed glass bottle, 20 cm. high and 12·5 cm. in diameter, which can be placed in an inverted position on any convenient stand. The bottom is cut off and is replaced by a cover of wood saturated with paraffin. The porous pot, P, of red clay is held in a ring of cork; it is 23·5 cm. deep, and has an inside diameter of 6 cm. It is essential that care should be used in the selection of a proper porous vessel, as the successful working of the cell depends in great measure on this. With the porous cells used by the author, the whole outside surface was thoroughly moist in one minute after the cell had been filled with water.

In the upper end of the porous pot is a cork thoroughly saturated with paraffin, through which passes the square end of the round carbon, K. This block is made from retort carbon, and is 22·5 cm. long and 3·5 cm. in diameter; it is bored out from below to a diameter of 1·5 cm. for a length of 18 cm., and at the top of the bore are a number of radial perforations. A glass tube, M, with funnel-shaped top, reaches up to these openings, and, below, it passes through the bottom of the clay vessel and the cork stopper at the mouth of the glass bottle. The bottom of the porous pot is covered with a coating of paraffin, as are also its upper edge and the top of the carbon. On the shoulder of the glass bottle rests a gutta percha ring, hollowed out in form of a gutter, $x\,y$, and filled with mercury, into which dips the lower end of the zinc cylinder, 16 cm. high, 6 mm. thick, and weighing 2 kg. Through the cork in the mouth of the bottle pass the two tubes, R and r, while through the wooden cover pass the thistle-tubes, t and t', the former reaching to the upper edge of the zinc, and the latter to the bottom of the porous pot.

The liquids circulate through the cell in the following way:—The fresh nitric acid flows through the thistle-tube t' down to the bottom of the porous cell, while the spent acid flows off through the side holes at the top of the carbon and the centre tube, M, into a receiving vessel placed below the cell. The dilute sulphuric acid enters above by the short tube t, and flows off below by the syphon tube R. The level of the two liquids differs slightly, as shown bp the dotted lines in the figure. S S is the cross section of a glass tube bent into a circle, through which a current of cold water passes in order to keep the liquid of the cell at a constant temperature. All the connections are made by means of mercury. The connection with the zinc cylinder is made by a copper wire, enclosed in a glass tube, passing through the wooden lid, and reaching down into the mercury in the gutta percha gutter in which the zinc stands, while the square end of the carbon is provided with a cup, Q, bored in it and filled with mercury.

The author describes the manner of mounting two or more cells, as well a number of measurements made with such a battery, and an ingenious form of mercury commutator for connecting the cells in different ways.

JOURNAL

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Telegraph Engineers and of Electricians.

Vol. XII. 1883. No. 48.

The One Hundred and Twenty-first Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 12th, 1883—Mr. Willoughby Smith, President, in the Chair.

The minutes of the last meeting were read and confirmed, and the names of new candidates were announced and suspended.

The Secretary announced the following donations to the Library:—Professor A. Graham Bell, Member; Dr. J. Hopkinson, F.R.S., Member; Mr. F. G. Teale, Member; Mr. S. M. Banker, Associate; Mr. T. Baxter, Herr A. Hartleben, the Direct Spanish Telegraph Company, and the Electrical Trading Company.

A unanimous vote of thanks to the donors was passed.

The following paper was then read:—

ON MICROPHONIC CONTACTS.

By Shelford Bidwell, M.A., LL.B.

It is generally admitted that the variation of the electrical resistance of two conductors in contact, under different degrees of pressure, was first observed by M. du Moncel, whose researches on the subject were published in 1856. This principle was applied ten years later by M. Clerac in the construction of rheostats, the resistance of which was varied by the pressure of a piston upon powdered carbon contained in an insulating tube. In July, 1877, Mr. Edison took out a patent for his celebrated telephone transmitter, in which the varying pressure of a tympanum or diaphragm upon a semi-conducting material was used to regulate the strength of an electric current in such a manner as to render it capable of reproducing speech in a suitable receiver.

On May 9th, 1878, a very remarkable paper was read before the Royal Society by Professor Hughes. It was entitled "The Action of Sonorous Vibrations in varying the Force of an Electric Current," and contained descriptions of a great number of experiments, which showed in effect that apparatus of the very simplest description, consisting merely of loose contacts of any substance which would conduct electricity, could be used, not only for transmitting speech, but for rendering audible in the telephone sounds which to the unassisted ear were quite inappreciable. It is to this latter property that the microphone, with which the name of Professor Hughes will always be associated, owes its title.

Though at the present time the matter is of great practical importance, there is much in the details of the behaviour of microphonic contacts which has not hitherto been fully investigated or satisfactorily explained; and it was for the purpose of throwing some little additional light on the subject, and if possible clearing up certain apparent anomalies connected with it, that the experiments described in this paper were undertaken.

Loose contacts are proverbial for the uncertainty of their action, and it was to be expected that the investigation would prove troublesome, and the results obtained by no means uniform. But by multiplying experiments, the element of uncertainty has been to a great extent removed, and several interesting facts, not, I think, previously noticed, seem to be clearly established.

The first requisite was an instrument for accurately measuring and regulating the pressure between the conducting materials. Several devices were tried, and successively abandoned as unsatisfactory. The arrangement which was finally adopted, and which was used for nearly all the experiments, is shown in Fig. 1. The apparatus consists of a little balance. H K is a light steel rod (formed of a fine knitting needle), balanced at C on a knife-edge which is fixed at right angles to it. To one end, K, is attached a split tube of thin copper for holding short rods of the different substances to be tested, which are held so as to press at right angles upon another rod, R, held in a spring clamp. Midway between C and K is hung the scale-pan, S, and at H is a sliding counterpoise, which at the beginning of an experiment is so adjusted that a weight of ·01 gramme in the scale-pan will just bring K and R into contact with each other. Electrical connection is made through the wires, W_1 and W_2, the former of which is attached to the clamp holding R, and the latter communicates with a mercury cup, into which dips a wire attached to the rod H K at the point C. It is evident that the pressure between K and R will be very approximately equal to half the weight in the scale-pan, S.

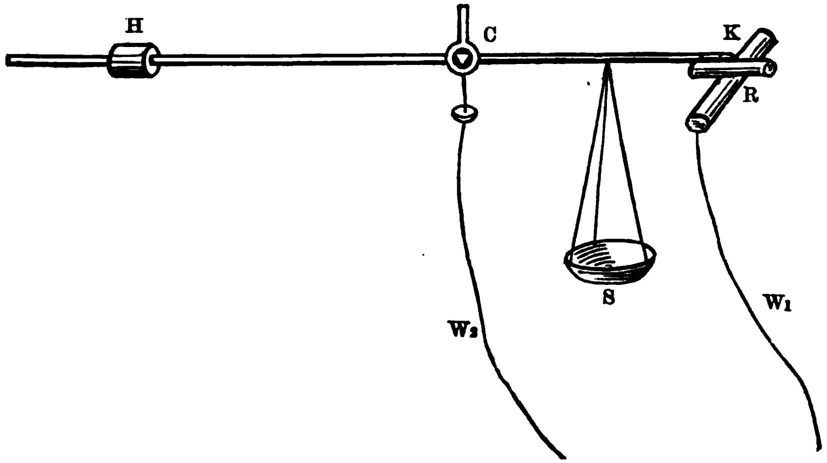

FIG. 1.

The object of the first experiments was to ascertain the effects of various pressures upon the resistance of two carbon rods in the positions shown at K and R, the measurements being made with a Wheatstone's bridge in the ordinary manner. The rods selected were electric light pencils of gas carbon, 6 mm. in diameter. In all, seventeen series of observations were thus made, the weights in the scale-pan varying from ·5 gramme to 100 grammes. The mean of three of these, which were performed with the same balance, and under as nearly as possible the same conditions, is given in Table I., from which was constructed the curve, Fig. 2, where the abscissæ represent the pressures at the point of contact, and the ordinates the resistance in ohms. The curve shows clearly that the greatest variations occur when the pressure is small and the resistance comparatively high. Thus, between ·25 gramme and ·5 gramme the resistance falls 5·1 ohms, while between 25 and 50 it only falls ·3 ohm.

In making these measurements the details of the arrangements were from time to time varied. Different proportional coils were used and different numbers of battery cells, and it soon became evident that the resistance of the carbon contact was largely dependent on the electro-motive force or the strength of the current used.

TABLE I.

Relation between the pressure of carbon contacts and their resistance when measured in the ordinary way.

Pressure.	Resistance.	Pressure.	Resistance.
Grammes.	Ohms.	Grammes.	Ohms.
·25	16·10	5	4·23
·5	11	7·5	3·53
1	8·43	10	3·06
1·5	6·70	12·5	2·80
2	6·15	15	2·60
2·5	5·90	17·5	2·46
3	5·13	20	2·33
3·5	5	25	2·16
4	4·60	50	1·86
4·5	4·33		

In Table II., giving the mean of two series of observations, is shown the result of using one, two, three, and four cells successively.

It became interesting to ascertain what would be the effect of making still further variations in the strength of the measuring current. A box of resistance coils was therefore inserted between the battery (2 Leclanché's) and the bridge arrangement, and, the weight in the pan remaining the same, various resistances were successively unplugged in the box. Table III. gives a few of the measurements with pressures of ·5, 2·5, 7·5, and 25 grammes.

This table shows that the resistance of the carbon contacts varies greatly with the strength of the current when the pressure is small, and but very slightly when the pressure is great.

It is clear, therefore, that the diminution of resistance which occurs when the pressure between carbon contacts is increased, is not under ordinary circumstances due solely to the increase of pressure; for the diminished resistance which results from the increased pressure causes an increase in the strength of the current, the effect of which is a still further fall in the resistance.

TABLE II.

Effect upon resistance of increased electro-motive force.

Pressure.	Resistance in ohms with			
	1 cell.	2 cells.	3 cells.	4 cells.
Grammes.				
·25	11·10	7·20	4·70	3·55
·5	5·95	4·70	4·10	3·50
1	4·40	3·65	3·25	3·10
1·5	3·60	3·20	2·95	2·80
2	3·55	3·15	2·80	2·50
2·5	3·35	2·95	2·65	2·40
3	2·90	2·55	2·35	2·30
3·5	2·45	2·30	2·05	1·95
4	2·25	2·10	2	1·90
4·5	2·10	1·95	1·85	1·75
5	1·95	1·85	1·75	1·70
7·5	1·55	1·55	1·50	1·55
10	1·50	1·45	1·40	1·35
25	1·15	1·05	1·05	1·05

Thus the total diminution of resistance is due only partly to increase of pressure, and partly also to increased strength of current.

An experiment was devised for exhibiting this effect in a conspicuous manner. The arrangement of the apparatus is shown in Fig. 3. a, na, nx, and x are the arms of an ordinary Wheatstone's bridge arrangement, B is a battery, and G a reflecting galvanometer. In the arm x is placed the carbon balance, S, and in the same arm a box of resistance coils, R; at a is a resistance of 2 ohms, and at *n a* 1,000 ohms. All the resistance being plugged out of R, the resistance of S, with a weight of 1 gramme in the scale-pan, was found to be 6·4 ohms. Two grammes were

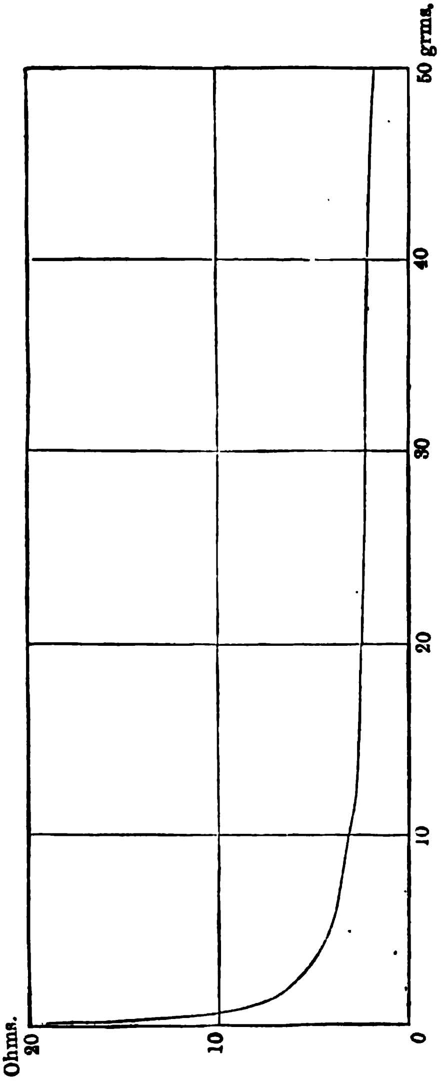

Fig. 2

then put in the scale-pan and the resistance fell to 5·1 ohms. In order to ascertain how much of this effect was due to increased strength of current, a resistance was inserted at R sufficient to reduce the current to its original strength—that is, in the present case, sufficient to make the total resistance, S + R, equal to the original resistance of 6·4 ohms. Now, in order to bring up the total resistance to 6·4 ohms, it was found necessary to insert at R, not 1·3 ohms, as might have been expected, but exactly 1 ohm. The resistance of the carbon alone was now (6·4 ohms — 1 ohm =) 5·4 ohms, a weight of 2 grammes being in the scale-pan, but the current the same as when only 1 gramme was in the scale-pan. The further reduction of 0·3 ohm when the measurement was made without the added resistance was due to the increased strength of current consequent upon the increased pressure.

TABLE III.

Effect of varying current by inserting resistance between the battery and the bridge.

Added resistance in circuit.	Resistance of contact in ohms with pressures of			
	·5 gramme.	2·5 grammes.	7·5 grammes.	25 grammes.
Ohms.				
0	5·6	2·7	1·7	1·02
5	—	2·8	1·8	1·03
10	6·9	2·9	1·8	1·03
50	7·7	3·1	1·9	1·03
100	8·6	3·1	1·9	1·03
500	9·3	3·1	1·9	1·03
1,000	9·7	3·1	1·9	1·03

A similar arrangement was then used for determining the resistance of the carbon contacts under gradually increasing pressure, but with a constant current. If E denote the electromotive force of the battery, and B its internal resistance, then the current through the arm containing S will be

$$\frac{n}{n+1} \times \frac{\mathrm{E}}{\mathrm{B} + \dfrac{n(a+x)^2}{(a+x)+n(a+x)}}$$

or

$$\frac{n\mathrm{E}}{(n+1)\mathrm{B}+n(a+x)}$$

From this it is easy to determine what resistances should be inserted in the several branches of the bridge in order to obtain a current through x (and therefore through the carbon contacts) of any definite strength. For example, suppose that the electromotive force of our battery is 5 volts, its internal resistance 4·5 ohms, that $a = 10$, $n = 100$, and $n a = 1{,}000$, and that we desire a constant current of $\frac{1}{10}$ ampère, then we shall have $\frac{1}{10} = \frac{500}{100\,(x + 10) + 101 \times 4{\cdot}5}$. Whence $x = 35{\cdot}5$ nearly. We therefore put a resistance of $(35{\cdot}5 \times 100 =)$ 3,550 ohms in the branch $n x$, and adjust the resistance in the box, R, until the galvanometer indicates a balance. We then know that the resistance of R and S together ($= x$) is 35·5 ohms, and that a current of $\frac{1}{10}$ ampère is passing through R and S. In order to find the resistance of S alone, it is simply necessary to subtract from 35·5 ohms the unplugged resistance in R. It will be understood, therefore, that the resistance in $n x$ remains unchanged throughout the experiment, the measurements being made by varying the resistance in R until a balance is obtained.

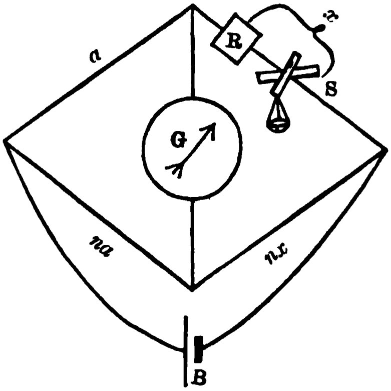

Fig. 3.

In Table IV. are given the results obtained with constant currents of ·1 and ·001 ampère, the pressure at the point of contact increasing from ·05 gramme to 25 grammes, all of them representing the mean of several series of measurements. It will be seen that with small pressures the resistance is largely dependent on the strength of the current, but when the pressure is considerable the resistance with weak and strong currents is nearly the same. This appears in the curves, Fig. 4.

The same effect may be exhibited in another form by varying the current when the pressure remains constant.

Table V. shows the variations in the resistance with currents of from ·01 ampère to ·2 ampère, the pressure at the point of contact being maintained at 1 gramme. The results are also shown in the curve, Fig. 5.

A few experiments were made in order to ascertain whether the diminution of resistance under pressure was entirely of a temporary nature, or whether it continued in any degree after the pressure had been removed. It was, of course, necessary to guard against the slightest disturbance of the carbon contacts, and it was found to be exceedingly difficult to remove weights from the scale-pan without risk of causing a material amount of vibration. The arrangement which was finally employed, and which proved to be fairly satisfactory, was as follows:—A 5-gramme weight was attached by a thread to one end of a horizontal lever and placed in the scale-pan, the lever being so fixed that when the other end of it was depressed the weight was raised about a centimètre above the pan: it could thus be taken out and replaced without causing any appreciable oscillation.

TABLE IV.

Effects of pressure upon resistance of carbon contacts with constant currents.

Pressure.	Resistance of contacts		Pressure.	Resistance of contacts	
	with ·1 ampère.	with ·001 ampère.		with ·1 ampère.	with ·001 ampère.
Grammes.	Ohms.	Ohms.	Grammes.	Ohms.	Ohms.
·05	11·02	68	4	3·63	5·70
·25	9·27	25·50	4·5	3·41	4·95
·5	8·45	17·75	5	3·31	4·95
1	6·56	11·75	7·5	2·95	3·65
1·5	5·53	9·75	10	2·51	3·15
2	5·34	7·50	15	2·10	2·45
2·5	4·57	6·50	20	1·89	2·10
3	4·15	5·85	25	1·67	1·75
3·5	4·03	5·70			

A weight of 1 gramme being in the scale-pan, the resistance of the contact was measured with the Wheatstone's bridge and one Leclanché cell; the 5-gramme weight was then lowered into the

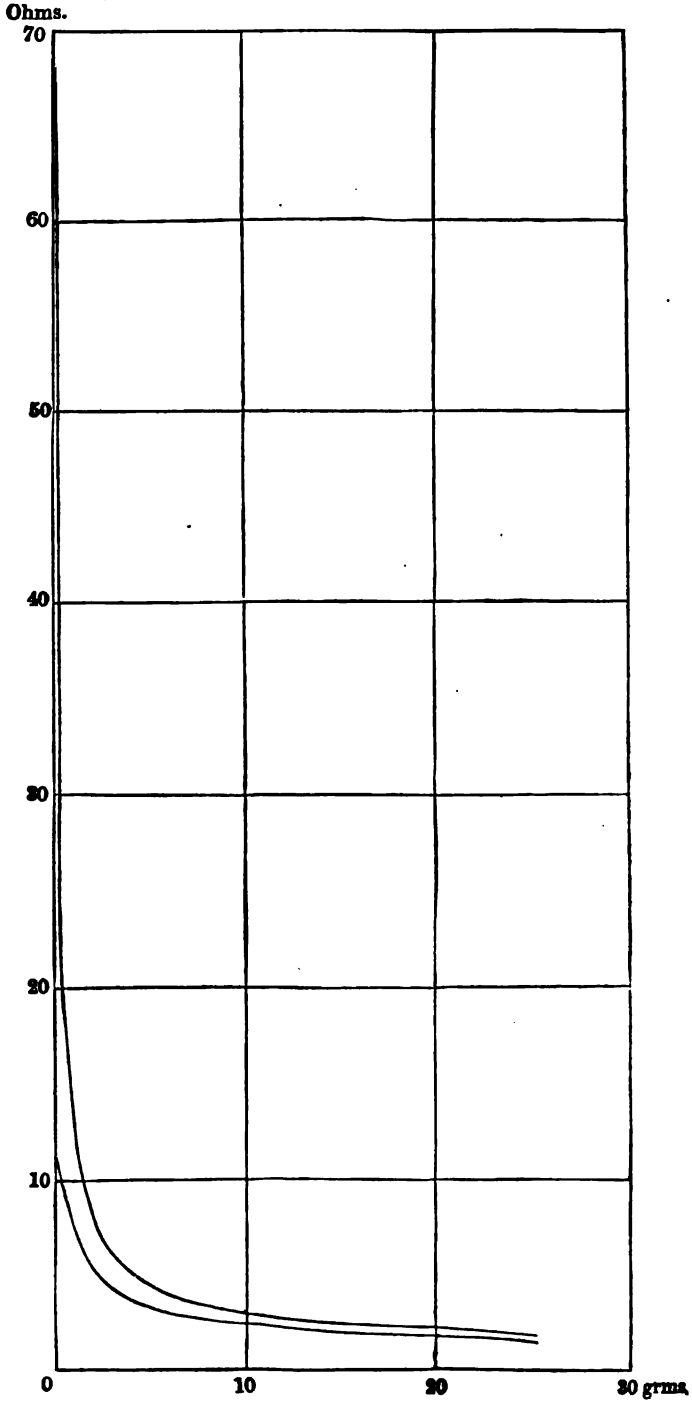

Fig. 4.

pan, and a measurement made of the diminished resistance. The 5-gramme weight was again gently raised, and the resistance with a single gramme once more measured. In every case it was found to have returned almost exactly to its original value. Table VI. gives six series of measurements thus made.

TABLE V.

Resistance of carbon contacts with different currents, the pressure remaining constant at 1 gramme.

Current.	Resistance.	Current.	Resistance.
Ampère.	Ohms.	Ampère.	Ohms.
·01	11·4	·07	8·5
·02	10·8	·08	8·2
·03	10·4	·09	7·9
·04	9·8	·1	7·6
·05	9·6	·15	5·6
·06	8·9	·2	4·5

The experiment was repeated with a resistance of 300 ohms between the battery and the bridge, that the current might be smaller. The results are contained in Table VII., and are

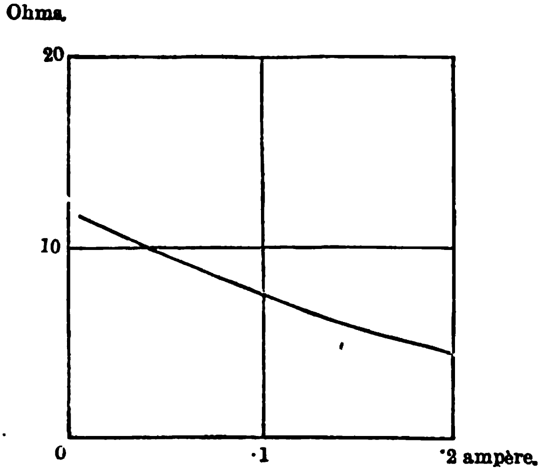

FIG. 5.

substantially the same as before. It will be noticed, however, that the final resistance is almost invariably slightly higher than the original resistance, but this increased resistance gradually diminished, as may be seen by comparing the last figure of one line with the first of the next, and perhaps, if time were given, it would return to its original value. There is little doubt that this is a thermo-electric effect.

Since, as is well known, the resistance of a continuous conductor of carbon is diminished by heat, it might perhaps be supposed that the diminution which the resistance of carbon contacts exhibits under the influence of increased currents is due simply to the heating effect of the current. Experiments made with the view of testing this hypothesis failed however to support it. The carbon balance, with a weight of 2 grammes in the scale-pan, was placed in an air-bath, the bulb of a thermometer being very near the points of contact; the temperature was slowly raised, and the resistance was from time to time measured in the ordinary way. At 16° (the temperature of the room) the resistance was 9 ohms. As the temperature rose the resistance varied irregularly, increasing, however, on the whole, and never becoming less than it was at first, until at 50° it had become 10·7 ohms.

TABLE VI.

Showing effect upon resistance of increasing and again reducing pressure.

One Leclanché. Proportional coils 10 and 1,000 ohms.

Pressure.	Resistance.					
Grammes.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
·5	19	20	20	19·6	19·3	19·2
3	15	15	14	13·9	13·7	13·6
·5	20	20	20	19·9	19·5	20

TABLE VII.

Same arrangement as in Table VI., with a resistance of 300 ohms between battery and bridge.

Pressure.	Resistance.					
Grammes.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
·5	30·1	31·5	31·6	34·4	35·4	34·1
3	24·3	25·2	19	19·6	19·8	18·8
·5	32·9	33·2	34·4	36·4	35·1	36·4

The experiment was repeated with a weight of 10 grammes in the scale-pan. The resistance at 17° was 4·7 ohms. As in the former case, the resistance rose and fell irregularly as the temperature increased until 31°, when it was 4·7 ohms—exactly the same as at first. From this point it increased with great steadiness and regularity to 5·7 ohms at a temperature of 50°.

It seemed possible that the irregularities in the results might be due to expansions and contractions of the steel beam of the balance, owing to which the upper carbon would be caused to slide over the lower, and thus form fresh points of contact of different resistances. Another arrangement was therefore made, which is shown in Fig. 6. Across a fixed carbon rod, 6 mm. in diameter, was suspended a smaller piece, having attached to it a wire of the shape shown in the figure, which dipped into a mercury cup. The total weight of the upper carbon and the wire was made equal to 1 gramme.

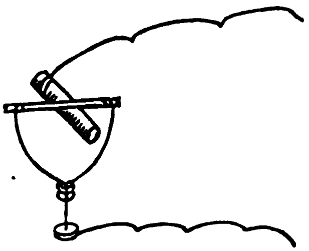

Fig. 6.

The resistance of this combination at a temperature of 15° was 11·9 ohms. When the bath was heated, the resistance at first began to fall steadily, becoming 11·4 ohms at 20°, but from this point it again rose, and with some irregularities continued to rise until a maximum of 15·6 ohms was reached at a temperature of 36°. The bath was then left to cool. As the thermometer fell, the resistance at first rose for a short time, being 16 ohms at 34°. Then it fell with great steadiness and regularity to 14·2 ohms at 24°, when the observations were discontinued. Other similar experiments were made, with equally inconclusive results. So far as they are worth anything, they tend to show that the resistance of a carbon contact is increased, instead of being diminished, by heat.* This may possibly be due to the formation of a non-conducting film by air or gases which the heat expels from the carbon, the effect of which might, however, be diminished or altogether destroyed at higher temperatures.

Under certain circumstances the current appears to produce a permanent and not merely temporary effect upon the resistance; that is to say, the resistance of the contacts is found to be lower (or higher) after the current has ceased than it was at first. The following are a few typical cases:—

A weight of one gramme being in the scale-pan, the resistance of the contacts with a small current (one or two milli-ampères) was 10·5 ohms. A current of ·15 ampères (measured by a tangent galvanometer) was then passed through the carbons for ten seconds. On afterwards testing with the small current, the resistance was found to have fallen to 5·8 ohms. The carbons were then separated by gently raising the scale-pan for a few moments, and carefully replaced in the same position as before. The resistance was now 10·6 ohms, or almost exactly the same as at first. With 5 grammes in the scale-pan, the same current reduced the resistance from 6·6 to 4·3 ohms. With 10 grammes the resistance was permanently reduced from 4·7 to 3·4 ohms, and with 50 grammes from 2·4 ohms to 2 ohms. All these effects were produced by a current of ·15 ampère. Stronger currents (up to a certain point) effect greater reductions.

But when the proportion of the current strength to the pressure at the points of contact exceeds a certain limit, the effect of the current is apparently to produce a permanent *increase* in the resistance, and this increase is sometimes very considerable. Thus, for example, with a weight of 5 grammes the resistance was 6 ohms; after a current of ·6 ampère (four times as strong as in the preceding experiments) had been passed through the carbons for ten seconds, the resistance, measured with a small current, appeared to be about 200 ohms, falling in three minutes to 35·9 ohms. These latter measurements, however, afterwards turned out to be partly illusory; for a tangent galvanometer, placed in circuit with the carbon, shows that the effect of a current beyond a certain strength is really to render the resistance infinite, or at least very great. The resistance does not rise gradually, but suddenly, generally after the circuit has been closed for from two to ten seconds, and at the moment of the rise a minute flash of light appears at the points of contact. The galvanometer needle suddenly goes to zero, and when this has occurred no deflection whatever is produced on opening and closing the circuit with a key. A slight vibration, however, partly or wholly restores the conductivity of the carbon, and, curiously enough, the same effect is often produced by the feeble current used to measure the resistance. Thus it may happen that, while the moment before measurement the resistance may be infinity, the very act of measurement reduces it to a few hundred ohms.

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* I take it as being beyond dispute that the resistance of a *continuous* carbon conductor is diminished by heat. A Swan lamp being placed in the air-bath, the resistance of the carbon filament steadily fell from 80·9 ohms at 19° to 76·5 ohms at 100°, following the received law.

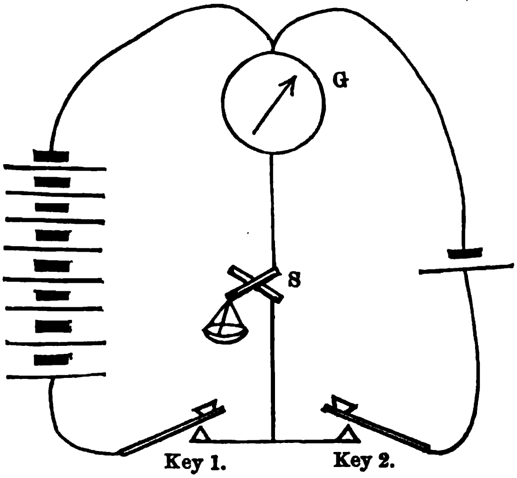

Fig. 7.

This curious effect is best shown by the special arrangement indicated in Fig. 7. G is the tangent galvanometer, S the carbon balance, in the scale-pan of which is a weight of one gramme. When key 1 is depressed, the current from 8 Leclanché cells passes through the galvanometer and the carbons. The immediate effect is a deflection of (in the case of the galvanometer used) about 75°. Almost instantly, however, the needle returns to zero, and key 1 may now be raised and depressed without producing the slightest movement of the needle. If, however, key 2 is depressed, it frequently happens that the current from the single cell will pass through the galvanometer and the carbons, producing a small deflection of from 2° to 10°. But if key 1 is once more depressed, the needle again altogether fails to move.* The resistance at S is therefore of a very peculiar nature, being more easily overcome by a small electro-motive force than by a high one.† Though I have successfully performed this experiment some scores of times, it occasionally happens that the single cell is as powerless to overcome the resistance as the larger battery.

The group of phenomena last described were now investigated systematically. In the first place the arrangement shown in Fig. 8 was used, the carbon balance, a tangent galvanometer, and a box of resistance coils being arranged in simple circuit with a battery of 10 Leclanché cells. Various weights were put in the scale-pan, and resistance was gradually taken out of R until the sudden rise in the resistance of the carbon contacts occurred, the galvanometer reading immediately before it went to zero being noted. The results are given in Table VIII., the pressure at the point of contact ranging from ·05 gramme, when the sudden rise occurred with ·02 ampère, to 5 grammes, when the rise occurred with ·63 ampère.

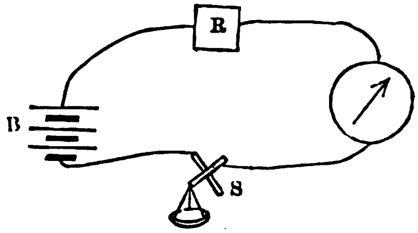

Fig. 8.

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* In order to avoid the effect of possible vibration, the keys should not be on the same table as the carbon balance.

† By means of a reflecting galvanometer this experiment was successfully shown at the meeting.

It will be remembered that before the sudden rise an increasing strength of current effects a permanent reduction in the resistance of the carbon. For examining this effect the apparatus was arranged as shown in Fig. 9. It is essentially a Wheatstone's bridge arrangement, the proportional coils used being 1 and 1,000 ohms. A tangent galvanometer of ·9 ohms resistance is inserted in the same arm as the carbon balance, and a box of resistance coils, R, is placed between the battery (which consists of 8 or 10 Leclanché cells) and the bridge. The arrangement admits of the measurement of the resistance of the carbon when traversed by strong currents, the strength of which can be regulated by the

TABLE VIII.

Showing for various pressures the smallest current which produced a sudden great rise in the resistance of the carbon contacts.

Pressure.	Smallest current with which rise of resistance occurred.	Pressure.	Smallest current with which rise of resistance occurred.
Grammes.	Ampère.	Grammes.	Ampère.
·05	·02	1	·40
·1	·05	1·5	·43
·15	·10	2	·47
·2	·19	3	·57
·3	·17	4	·51
·4	·29	5	·63
·5	·37		

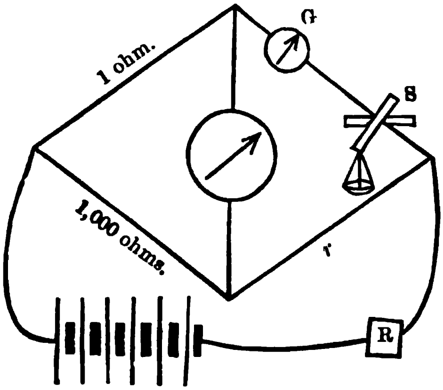

Fig. 9.

resistance box, R, and measured by the tangent galvanometer, G. A given weight being in the scale-pan, known currents of gradually increasing strength were passed through the carbon. The resistance was measured, 1st, while the known current was passing; 2nd, with a very small current, obtained by unplugging 10,000 ohms in the resistance box. As the currents increased in strength, the resistance, measured with the very small current, gradually diminished until a point was reached—and the lighter the weight the sooner this was the case—at which the resistance suddenly became very great. The measurements are given in Table IX., the currents varying from ·01 to ·7 ampère, and the pressures from ·05 gramme to 5 grammes.

TABLE IX.

Showing for different pressures the effect of increased current in first permanently reducing the resistance of the carbon contacts, and after a certain point (depending upon the pressure) greatly increasing it.

R_1 = Resistance in ohms with current in first column of table.

R_2 = Resistance with very weak current, obtained by inserting 10,000 ohms in the circuit.

Current.	Pressure ·05 gramme.		Pressure ·5 gramme.		Pressure 2·5 grammes.		Pressure 5 grammes.	
	R_1	R_2	R_1	R_2	R_1	R_2	R_1	R_2
Ampères. .01	38	62	10·2	10·3	4·8	4·8	2·60	2·6
.05	21	38	9·7	10·1	4·6	4·7	2·54	2·6
·1	Inf.		8·1	9·3	4·4	4·7	2·37	2·5
·2			6·3	8·4	4	4·5	2·23	2·4
·3			Inf.		3·7	4·3	2·11	2·3
·4					3·6	4·2	2·01	2·3
·5					Inf.		1·95	2·2
·6							1·90	2·2
·7							Inf.	

NOTE.—The resistance marked "Inf." was so great that the needle of the tangent galvanometer was not deflected from zero.

The experiments made with metals were less numerous than those with carbon. Metallic loose contacts were found to be even more uncertain and capricious in their action, and to obtain a really fair average of results a very great number of measurements would be required. Under precisely the same conditions, as nearly as they could be reproduced, the results obtained were frequently very different. Nevertheless, it has been possible to bring out with sufficient distinctness some remarkable peculiarities in the behaviour of metallic contacts.

The metal principally used was bismuth, which was selected on account of its high specific resistance, but a few experiments were also made with copper and platinum. The cylinders of bismuth and copper were of nearly the same diameter as those of the carbon previously used—6 mm. The platinum was prepared by soldering wires of that metal upon cylinders of brass. The balance already described and shown at Fig. 1 was used in the same manner as before.

The first unexpected phenomenon which attracted notice in working with bismuth was the very great permanent fall in the resistance which was produced by an ordinary current with light pressures. Using a single Leclanché cell, the lightest contact—the pressure being certainly less than ·01 gramme—failed to give a higher resistance than 9 ohms; but with 400 ohms inserted between the battery and the Wheatstone's bridge, a pressure of ·35 grammes gave a resistance which on different occasions varied between 73 and 110 ohms. When once, however, the resistance has been reduced by a strong current, the changes which occur when feebler currents are used are comparatively small, unless, of course, the points of contact are first separated. Of the nature of such changes I shall say more presently. In Table X. are given a few measurements, using one Lechanché cell—(1) with a resistance of 300 ohms between the cell and the bridge; (2) without the 300 ohms. It will be understood that the bismuth cylinders were separated from each other every time the weights were changed.

The effect in question is most marked with small pressures; with comparatively high pressures it quite disappears.

This phenomenon is accompanied by another. After the passage of the current which has caused the resistance to be so remarkably diminished, the bismuth cylinders are found to adhere to each other, a quite appreciable force being necessary to separate them. This effect, which is common to all metals, has been thoroughly investigated by Mr. Stroh, a description of whose experiments is given in the "Journal of the Society of Telegraph Engineers," Vol. IX., page 182. I shall therefore say nothing more about it, except that I have succeeded in causing two pieces of very thin platinum wire to stick to each other with a current of about a milli-ampère. Mr. Stroh attributes the effect to fusion.

A third apparent anomaly is the following:—A *small* weight being in the scale-pan, let a weak current be caused to pass through the points of contact: the resistance will be found to be high. Let a strong current be passed, and the resistance will be greatly diminished. Once more let the current be reduced to its original strength: the resistance will not rise to its original value; it will not, in fact, rise at all; it will be still further diminished. Table XI. gives examples of this experiment with pressures of ·5, 1, and 5 grammes. In the last case there is no change after the first fall of resistance.

TABLE X.

Bridge measurements of the resistance of bismuth contacts under different pressures.

Pressure.	Resistance of bismuth contact with 300 ohms between battery and bridge.	Resistance when the 300 ohms are removed.
Grammes.	Ohms.	Ohms.
·5	60	1·9
2·5	37·2	1·4
5	5·4	1·7
7.5	7·7	·2
10	·8	·8
15	·6	·5
25	·14	·14

Table XII. further illustrates the effect with greater variations of current and of weight, but in the experiments to which it relates the strongest current was used at first. As before, the bismuth cylinders were separated only when the weights were changed.

In order that this experiment may be successful, it is necessary that the surfaces in contact should be absolutely clean.

TABLE XI.

Showing variations produced in the resistance of bismuth contacts by using small and large currents alternately.

Current.	Resistance of bismuth contact with pressure of		
	·5 gramme.	1 gramme.	5 grammes.
Ampères.	Ohms.	Ohms.	Ohms.
·01	8	2·12	1·50
·5	1·27	1·27	1·22
·01	1·22	1·21	1·22
·5	1·27	1·26	1·22
·01	1·22	1·21	1·22

Unless they are scraped immediately before the measurements are made, the opposite effect may result—that is (after the first great fall of resistance), diminished current may produce increased resistance.

TABLE XII.

Effect of varying current upon the resistance of freshly scraped bismuth contacts.

Current.	Resistance of contact in ohms with pressure of				
	·25 grm.	·35 grm.	·5 grm.	1 grm.	2 grms.
Ampères.					
·5	1·52	1·39	1·55	1·33	1·35
·3	1·40	1·35	1·49	1·30	1·33
·1	1·37	1·33	1·45	1·28	1·32
·05	1·35	1·33	1·44	1·28	1·32
·01	1·18	1·32	1·41	1·28	1·32
·05	1·55	1·33	1·46	1·29	1·32
·1	1·72	1·34	1·47	1·29	1·33
·3	1·79	1·39	1·55	1·31	1·34
·5	1·49	1·43	1·61	1·33	1·35

REMARK.—With *clean* bismuth, decrease of current strength is accompanied by decrease of resistance.

Table XIII. gives the measurements, with the same currents as before, when the bismuth was first rubbed with the finger, instead of being scraped as had been done in the previous experiments.

I think these effects admit of a simple explanation. When the surfaces of the bismuth are clean, contact takes place entirely through the metal. The current heats the metal at the points of contact to an extent which depends partly upon the current strength; and the resistance, in accordance with the general law, increases with the temperature; strong currents will therefore give higher resistance than weak ones. When, on the other hand, the surface is not clean, a film of oxide or some foreign substance is interposed, the resistance of which, like that of carbon, is higher with a weak current than with a strong one.

TABLE XIII.

Effect of varying current upon the resistance of bismuth contacts when the metal has been rubbed with the finger.

Current.	Resistance of contact with pressure of		
	·35 gramme.	·5 gramme.	1 gramme.
Ampères.	Ohms.	Ohms.	Ohms.
·5	1·30	1·80	1·22
·3	1·75	1·46	1·28
·1	1·92	1·69	1·29
·05	1·83	1·92	1·29
·01	1·95	2·29	1·29
·05	1·89	2·07	1·28
·1	1·87	1·98	1·28
·3	1·77	1·72	1·27
·5	1·46	1·45	1·27

REMARK.—When the metal is *not perfectly clean*, decrease of current strength is accompanied by *increase* of resistance.

It is probable that similar effects occur with metals of lower specific resistance than that of bismuth; but their observation is very difficult, and requires more delicate apparatus than that at my disposal. Thus, of 40 measurements made with platinum contacts, 19 results were favourable to the theory, 10 adverse to it, and 11 neutral. With copper the indications were even more uncertain.

The resistance of bismuth contacts at various pressures was then measured with fixed currents of ·1, ·01, and ·001 ampère, the method used being the same as in the case of carbon. The mean of several series of such measurements is given in Table XIV., which reveals the existence of a more or less definite law, though the results of the experiments did not agree very closely among themselves.

Repeating with bismuth the experiments already described in the case of carbon, it was clear that the diminution of resistance effected by pressure is generally of a permanent nature, continuing to a great extent after the pressure has been removed (so long as the points of contact remain undisturbed), and thus reversing the case of carbon. With strong currents the variations of resistance were so uncertain and irregular that accurate measurements were impossible; but with a resistance of 300 ohms between the battery and the bridge the effect is very clearly marked. The results of the experiments thus made are contained in Table XV.

TABLE XIV.

Effect of pressure upon resistance of bismuth contacts with constant currents.

Pressure.	Resistance of contacts with		
	·1 ampère.	·01 ampère.	·001 ampère.
Grammes.	Ohms.	Ohms.	Ohms.
·05	5	20·82	182
·1	2	16·92	143·3
·15	2·5	14·60	97·3
·2	3·9	12·60	46
·25	2·6	10·05	41·6
·3	2·4	6·35	16·9
·35	2·1	4·12	21·6
·4	1·9	2·37	30·6
·45	1·5	1·60	18·3
·5	1·45	1·47	3·3
1	·95	1·35	...
1·5	·85	·70	...
2	·90	·62	...
2·5	·70	·30	...
3	·50	·15	...
3·5	·55	·07	...
4	·35	·05	...
4·5	·25	...	...
5	·15	...	...

The points of contact were of course changed for each series of observations, and it should be noted that in the case of the last three (which were not made on the same day as the others) I was less successful in arranging the lever so as to secure perfect freedom from oscillation when the weight was changed.

At the meeting of this Society, held on March 8th, Mr. Stroh gave an account of a method which he had devised for observing the movements performed by the horizontal carbon rod of an ordinary microphone when the instrument is in action. He attached a small mirror to the carbon, from which a beam of light was reflected to a wall, the distance of which was such that the movement of the spot of light was 2,000 times greater than that of the carbon at the point of contact, and it was found that if a current from three bichromate cells was passed through the microphone, when it was so adjusted as to be in the best working condition, the spot of light remained steadily deflected downwards, moving through a space of one millimètre, and returning to its former position when the current was interrupted. This, Mr. Stroh says, in an article which he has since published,* "seems to indicate that during the time when the carbons are really in what is called microphonic contact they are not in contact at all, or, at all events, there is a repellent action at the point of contact."

It occurred to me that additional light might be thrown upon some of the phenomena which I have investigated by the applica-

TABLE XV.

Showing great permanent effect of pressure upon the resistance of bismuth contacts.

Pressure.	Resistance of bismuth contacts.					
Grammes.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.	Ohms.
·5	10·9	37·5	16·3	14·5	25·5	62
3	1·3	1·7	·5	1	1·1	1·8
·5	1·5	1·7	·5	4·5	5·5	15

NOTE.—In the last three cases the apparatus for removing the additional weight worked badly, and caused a slight vibration of the scale-pan.

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* *Journal Society of Telegraph Engineers and of Electricians*, Vol. XII., No. 472, p. 144.

tion of Mr. Stroh's method, and I began by repeating the experiment almost exactly as he describes it. I at first found, as he did, that the effect only occurred when the microphone was so adjusted that the ticking of a watch laid upon the base could be distinctly heard in a telephone which was in the circuit. But with the high electro-motive force of three bichromate cells it was exceedingly difficult to get the microphone into good working condition, or to keep it so for any length of time. A tangent galvanometer was then inserted in the circuit, and it at once became evident that when the condition of the microphone was at its best the current was strongest. It appeared probable, therefore, that the magnitude of the apparent repulsive effect depended merely upon the strength of the current which was passing at the moment, and that the excellence of the microphonic effect had in itself nothing to do with the matter. The upper carbon was therefore pressed down with a weight too heavy to admit of the ticking of the watch being heard, and a steady current was thus secured without any difficulty. It was now found that the repulsive effect was almost, if not quite, as great as it was when the carbons were in loose contact, and little doubt was left that the phenomenon was merely a case of expansion due to the heating effect of the current. Cylinders of bismuth were then substituted for the carbon rods, and a similar effect was produced, the movement of the spot of light being somewhat greater than it was with carbon. With copper the apparent repulsion was rather less than with carbon, but perfectly well marked. Lead was then tried, being chosen on account of its high coefficient of expansion and its inferior conductivity for heat, and, as was expected, the deflection of the spot of light was very considerable.

One or two other curious results are worth noting. In the case of metal contacts, when the pressure was slight, the first effect of a small current was to produce closer contact; as the strength of the current was gradually increased, the contact became more and more close; when the circuit was broken, the Stroh effect occurred, and the contact became apparently still closer. This observation is evidently connected with the fact already discussed, that a current through loose metallic contacts causes a permanent decrease in their resistance. The same thing occurs when carbon contacts are used, but it is well marked only when the contact is exceedingly light, and is, I believe, due to the combustion of the minute projecting particles upon which the carbon rod rests. Finally, an attempt was made to exaggerate the effect by multiplying the number of carbon contacts. Twelve small pieces of carbon shaped like a whip-top were arranged in an upright tube of ebonite, which they loosely fitted. The lower carbon was fixed, and the upper one had a knife-edge upon which rested a horizontal carbon rod with a mirror attached. A current through this arrangement produced a deflection five or six times as great as in the case of the single contact. It now became possible to observe that the whole effect was *not* absolutely instantaneous as it had previously appeared to be, a slow movement of the image in the upward or downward direction continuing for two or three seconds after the circuit had been opened or closed. When a single long piece of carbon was substituted for the small pieces, the effect was similar, but not so great.

The little carbon balance (Fig. 1) was joined up in circuit with the apparatus just described, and it was observed that with a continuous current the magnitude of the repulsive effect could be varied by altering the weights in the scale-pan. This experiment was much more effective when an Edison carbon telephone was used instead of the balance. Pressing the diaphragm with the finger caused the spot of light to move to an extent depending upon the pressure.

It naturally occurred to me that a *receiving* microphone might easily be made by slightly modifying the apparatus to which the mirror was attached. A dozen top-shaped carbons were placed end to end in a glass tube: the bottom one was sealed in, and attached by a wire to a binding screw; the upper one, which was connected by a wire to a second binding screw, was made to press gently upon the centre of a míca diaphragm, on the other side of which was a steel spring to increase its elasticity. Using 12 Leclanché cells and an Edison transmitter, it was found quite possible to carry on a conversation, and very little adjustment was necessary. The instrument of course operates through expansion by heat, the action being exactly similar to that of the platinum wire in Mr. Preece's thermal telephone receiver; and I have little doubt that the operation of all receiving microphones which have been constructed may be explained in the same way.

Application to the Microphone.—The results of the experiments described in this paper may perhaps throw some light on a matter which has never hitherto been fully explained. Why does carbon give far better results than any metal when used in the microphone? It seems to me that this question may be very easily answered. The mere fact that a current causes delicately adjusted metal contacts to adhere to each other is sufficient to account for the superior efficiency of carbon. It is possible, indeed, to transmit articulate sounds by means of a metal microphone, as is proved by some of Professor Hughes's earliest experiments, but the action of such an instrument, as compared with one of carbon, is extremely uncertain and imperfect. This is no doubt due to the fact that, although vibrations corresponding to the fundamental tone are easily produced, the minute superimposed vibrations due to the upper partials, upon which depends the distinctive character of a particular sound, are, owing to the adhesion, very imperfectly represented, if not entirely lost.

At a recent meeting of this Society, Mr. Munro exhibited a metallic microphone, in which, by the ingenious use of iron gauze, the current is divided among a great number of points, at each of which its heating effect is small, and adhesion therefore does not occur to any perceptible extent. But though this instrument may be good enough for practical use, Mr. Munro would hardly contend that its performance is equal to that of a good carbon microphone.

In addition to this phenomenon of adhesion, and probably connected with it, are the facts that metallic contacts, unlike those of carbon, do not even approximately recover their original resistance when once it has been reduced by increased pressure or increased current, unless, indeed, complete separation occurs, and even the initial effect of pressure upon resistance is in general much more marked with carbon than with metals.

Lastly, there is the fact that, in the case of carbon, pressure and current act in consonance with each other: pressure diminishes the resistance, and in so doing increases the strength of the current, and the current thus strengthened effects a further diminution in the resistance. In the case of metals, on the other hand (or at least in the case of clean bismuth), pressure and current tend to produce opposite effects. The resistance is diminished by pressure, and the current consequently strengthened, but by reason of the increased strength of current the resistance is *higher* than it would have been if the current had remained unchanged. The effect of this antagonism is not very great, but it seems to give a material advantage to carbon.

In conclusion, I think that the results arrived at in this paper may be made to furnish data for designing upon scientific principles a carbon pencil microphone which shall possess the maximum degree of efficiency; and, without entering into details or making any specific calculations, I propose to indicate briefly the lines which should in my opinion be followed in order to secure the most satisfactory results. The following questions present themselves for solution:—

(1.) Should the constituent elements of the microphone be few or many?

(2.) Should they be arranged in series or in multiple arc, or in a combination of the two?

(3.) Should they be light or heavy?

(4.) Should the pressure at the points of contact be great or small?

(5.) Should the resistance of the system be high or low?

(6.) Should the current used be strong or weak?

Before considering the points here raised, it may be well to state definitely what I conceive to be the *modus operandi* of the microphone as used for the transmission of speech.

Fig. 10 is intended to represent a typical microphonic element. A horizontal carbon pencil, A B, is supported at the ends upon two carbon blocks, C, D, which are attached firmly to a sound-board, usually of thin pine. The sound-board is acted upon by air waves, and caused to repeat upon a smaller scale the movements of the air particles which beat against it. It carries the lower carbon blocks with it in its movements, and causes them to press with varying force against the under side of the loose carbon pencil. The varying pressure produces alterations in the resistance at the points of contact, in exact correspondence with the phases of the sound waves, and the strength of a current passing through the system is thus regulated in such a manner as to fit it for reproducing the original sound in a telephone. The loudness of the repeated sound increases with the limits between which the current is made to vary.

Now, if it is true, as this theory supposes, that the whole action of the microphone is due to the motion, or tendency to move, of the lower carbon blocks (the influence of sonorous vibrations upon the loose block or pencil being so small as to be negligible), it follows that, if the variations of pressure are to be considerable, the upper block must be capable of offering resistance to the movements which the lower blocks tend to perform. It will, of course, do this the more effectively, the greater its inertia, and therefore its mass or weight. In order, therefore, to secure the greatest variations of *pressure*, the loose carbon must clearly be heavy.

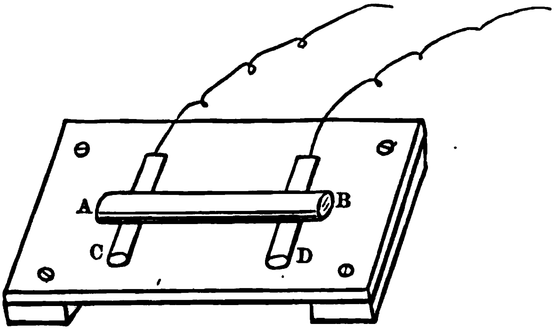

FIG. 10.

But with the ordinary arrangement a heavy carbon implies great pressure at the points of contact; and it has been clearly proved that the resistance is far more easily varied when the average pressure is small than when it is great. Here, then, we have two conflicting requirements, and, if the usual arrangement is to be retained, a compromise must be effected, and it will be necessary to ascertain by experiment or calculation what must be the weight of the carbon in order that sonorous vibrations may produce the widest variations of current; remembering that if it is too light the differences of *pressure* will be small, and if too heavy the changes of pressure will give but small changes of *resistance.* But it would, I think, be decidedly better to depart from the usual form, the chief recommendation of which is its extreme simplicity. Many methods will suggest themselves of arranging the materials in such a manner that, while the weight of the loose carbons is great, they may exert but slight pressure against the fixed carbons. Fig. 11 illustrates an example of such an arrangement. The loose carbons are suspended vertically upon a wire (attached to the frame of the instrument) which passes through holes in their upper extremities, their other ends resting lightly upon a horizontal pencil which is fixed to an upright sound-board. This may be mounted in a swing-frame like that of a looking-glass, and the pressure of the carbons adjusted by varying the inclination of the frame. Ordinary ingenuity will suggest other methods of accomplishing the same purpose.

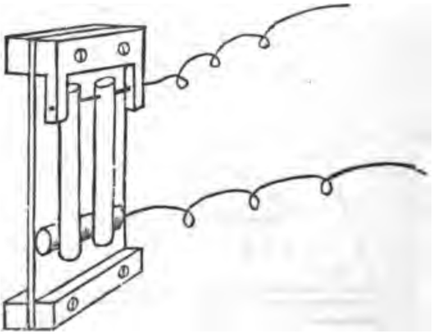

FIG. 11.

The question as to whether it is preferable to arrange the elements in series or in multiple arc is very easily answered. If the elements are in series, the whole of the current used will pass through each of them; while if they are in multiple arc the current will be subdivided and each element will be traversed by a current the strength of which (supposing them all to be equal) will be inversely proportional to the number of elements. Now, the sensitiveness of the contacts to changes of pressure has been shown to increase greatly with small currents, and hence it follows that the multiple arc is to be preferred to the series system. The same reasoning, in a modified form, will condemn a system in which the series and multiple arc arrangements are combined; for the greatest perfection, therefore, the multiple arc system pure and simple should be adopted.

The high degree of sensitiveness attending the use of small currents points also to the advantage of multiplying the number of the elements. I believe that this number might well be made much greater than is customary, being limited only by considerations of space. The sound-board should not, of course, be too large, or there will be a risk of the sonorous waves reaching different parts of it in materially different phases, especially if their direction is oblique to the surface of the board. But it would not be difficult to arrange 50 or 60 such suspended elements as those shown in Fig. 11 within an area 15 centimètres square. I have made a transmitter upon this plan, in which the suspended elements, forty-five in number, consist of brass tubes filled with lead, and having short pieces of carbon fitted into their lower ends, and I believe its performance is greatly superior to that of most instruments of the kind.*

Since the efficiency of a microphone depends principally upon its power of varying the strength of the current in a circuit comprising the microphone, a battery, and the primary wire of an induction coil, it is clear that the electro-motive force of the battery should be as high as is consistent with other conditions, and the resistance of the battery coil and microphone should be low; for the proportional change of resistance caused by sonorous vibrations remaining the same, the stronger the maximum and minimum currents, the greater will be the actual difference between them. But for securing sensitiveness it is very important that the current passing through the microphone should not exceed a certain strength, and indeed, if the pressure is light (as it should be), a strong current will, as we have seen, enormously increase the resistance of the contacts, or some of them, thus impairing or utterly destroying the efficiency of the instrument.

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* Since this was constructed it has come to my knowledge that M. Boudet de Paris has devised a somewhat similar transmitter, which is thus described by him in *La Lumière Électrique*, 28th September, 1881:—"Une membrane de caoutchouc durci porte à son centre un axe coudé sur lequel sont fixées transversalement deux tiges cylindriques de charbon. Au-dessus de ces deux tiges sont suspendues côté à côté 24 tiges de cuivre (en 2 séries de 12), terminées par un petit cylindre de charbon à l'extrémité duquel est vissée une masse métallique pesante. L'appareil est renfermé dans un anneau cylindrique dont le fond en glace permet de voir la disposition des différentes parties. Enfin le tout est fixé sur un pied support, et l'inclinaison est obtenue au moyen d'une charnière; pour plus de précision, le jeu de cette charnière est commandé par une vis à pas très fin. On remarquera en outre que, dans cet appareil, la charge des charbons mobiles étant placée au-dessous de leur point de contact, et très près de lui, il suffit d'une variation très faible de l'inclinaison pour faire varier la pesanteur et pour obtenir des pressions très différentes."

The subjects of all the questions with which these observations were introduced have now been touched upon, though no attempt has been made to answer them very definitely. In general we may say that—

(1.) The constituent elements of a microphone should be numerous.

(2.) They should be arranged in multiple arc.

(3.) They should be heavy.

(4.) The pressure at the points of contact should be light.

(5.) The resistance of the microphonic system should depend upon the resistance of the rest of the circuit (which should be small) and upon its sensitiveness to changes of pressure. In general it should be small.

(6.) Up to a certain limit, depending upon the number of contacts and their pressures, the current used should be strong.

It need hardly be added, in conclusion, that the observations which I have ventured to offer do not profess to do more than indicate the merest outlines of the details which should be considered in designing a carbon pencil microphone of the greatest possible perfection.

The President: It will be remembered that at our last meeting the discussion on Mr. Munro's paper was deferred until after the one just read by Mr. Shelford Bidwell, as both appertained to the same subject; but now a third paper has been presented on the action of the microphone, and therefore, to prevent confusion, it will be better to read it at once, so that the discussion following may embrace all three papers.

The following paper was then read:—

NOTE ON THE INFLUENCE OF SURFACE-CONDENSED GAS UPON THE ACTION OF THE MICROPHONE.

By I. Probert and Alfred W. Soward, Associates.

One of the theories advanced to account for the action of the microphone states that the layer of air between the microphonic surfaces acts as an ordinary resistance; that the effect of sonorous vibrations is to cause the carbons to approach and to recede from one another, and so to alter the thickness, and consequently the resistance, of the interposed layer. This variation of resistance produces the variation of current essential for the reproduction of sound.

There is in physics a phenomenon known as heterogenous adhesion, one phase of which is the surface condensation of gases. A study of this phenomenon shows that every surface exposed to a gas is coated with a condensed layer of that gas. The more readily the gas is liquefiable by pressure, the greater is the quantity condensed; and in the case of the very readily liquefiable gases the exposed surface is actually coated with a layer of liquid.

Our atmosphere, as is well known, consists mainly of nitrogen, oxygen, carbon dioxide, and water vapour. Careful experiments with charcoal (a substance which, owing to its porous nature, possesses a very large surface relatively to its mass) show that the coefficients of surface condensation of these gases (that of hydrogen being taken as unity) are 3·5, 4, 15, and 80 respectively. These figures, multiplied by the figures representing the proportions in which the gases exist in the air, viz., 79, 21, ·03, and (say) 1·5, give the proportions in which the substances exist in the condensed layer. They are—nitrogen, 276·5; oxygen, 84; carbon dioxide, ·45; and water vapour, 120. Of these four gases, the first two, oxygen and nitrogen, would be condensed without liquefaction, and the effect of this condensation would be to slightly reduce the electrical conductivity of the layer; for it is known that the conductivity of a gas is decreased by condensation. The carbon dioxide would probably be liquefied, and the effect of its liquefaction would be (slightly, on account of the small quantity acting) to increase the conductivity; the water vapour would certainly be liquefied, and the effect of its liquefaction would be to very considerably increase the electrical conductivity of the layer. The effect, then, of this phenomenon of surface condensation is to coat the microphonic surfaces with a layer of far better electrical conductivity than ordinary air.

In order to test the influence of this layer of condensed air upon the action of the microphone, we have lately made some experiments with the instrument in different gases. The gases used were hydrogen, carbon dioxide, wet air, and dry air, and our method of experimentation was as follows:—

A microphone of a well-known pattern, consisting of a glass tube containing cylindrical carbon blocks, and having many contact surfaces, was attached to the inside of a clock-case, and placed in circuit with a Hughes audiometer, a Morse key, and three Leclanché cells. The carbon blocks of the microphone were pressed well together, so as to avoid, as far as possible, alteration of contact due to chance vibrations. A current of air, dried by passage over pumice stone soaked in strong sulphuric acid, and then over pentoxide of phosphorus, was urged through the microphone for half an hour. The resistance of the microphone was then measured, and the point of the audiometer scale determined at which the beating of the clock became inaudible in the telephones. A current of similarly dried hydrogen was then passed through the microphone for half an hour, and the resistance again measured, and the point of silence determined in the audiometer. The experiment was next repeated with carbon dioxide, the dried gas being passed through the microphone for a similar period; and, finally, for fifteen minutes a current of air was forced through water contained in a Woulfe's bottle (in order to load the air with water vapour), and then through the microphone. The resistances and points of silence in the audiometer were determined as before. Our results are expressed in the following table, and it may be noted that the scale of the audiometer was graduated from 0° in the centre to 100°:—

Name of gas.	Resistance of microphone.	Point on scale of audiometer at which silence was reached.
	Ohms.	
Wet air	886	51°
Carbon dioxide	435	56°
Hydrogen	600	58°.
Dry air	520	68°

It will be seen that the best result was obtained with wet air, which calculation shows should give a good conducting surface layer. Next in order is carbon dioxide, which in the liquid state is a moderate conductor. Hydrogen comes next, as would be expected, for neither it nor dry air is reducible to the liquid state by ordinary surface condensation, and the best result is to be looked for from the less condensable of the two—that is, hydrogen. The resistance of the hydrogen-charged microphone is, however, anomalous.

In order to obtain our layers of condensed gas, we considered it sufficient to pass a stream of the desired gas over the microphone for some time, because it is known that when a piece of carbon charged with one gas is placed in an atmosphere of another, the two gases diffuse into one another, with a result that the carbon remains charged with a mixture in the proportions indicated by multiplying their percentage volumes by their respective condensation coefficients. In our experiments the quantity of gas passed through the microphone in any one experiment was vastly greater than the residual gas from previous experiments.

We did not attempt to rigorously exclude water in any of our experiments; for it has been shown that the last trace of water so obstinately clings to a surface, that to perfectly dry a glass tube it must be raised to the softening point, and so kept for some hours while a stream of dry air is urged through it.

In these experiments with different gases there is a possible source of error which must not be overlooked. We have elsewhere shown (*Chemical News*, Vol. 47, p. 157) that the resistance of a piece of porous carbon is not a constant for a given temperature, but varies with the chemical nature and with the density of the gas with which the pores of the carbon are filled. It follows from this, that if a constant electro-motive force be used, the current flowing and the sounds obtained will be altered by any alteration in the nature of the gas absorbed in the body of the microphone, irrespective of the contact surfaces. But the carbon used for our microphone had very little absorptive power, and its resistance was practically constant at constant temperature.

We think from these experiments it is fair to infer that the layer of condensed gas with which every microphone surface is cevered is concerned to some extent in the regulation of microphonic action.

As bearing upon the effect of a layer of moisture we may notice the following experiment:—

A common tin canister, joined through a rheotome to one pole of a 4-cell Grove's battery, and held in the hand by an insulating handle, was pressed against the ear; a wire attached to the second pole of the battery was held against the tongue. Sounds were heard corresponding to the working of the rheotome, and a burning sensation was experienced at the ear, such as one might imagine would be produced by innumerable small electric discharges. Similar results were obtained with a 10-cell Daniell's battery (chamber pattern). The surface of the canister, which, having been exposed to the air, was coated, as all similarly exposed surfaces are, with an invisible layer of moisture, was then well wetted. The burning sensation became more marked, but the sounds of the make and break were no longer audible. We may add that a solid brass ball, whether wet or dry, gave no sounds in this experiment.

A hearty vote of thanks was unanimously accorded to Messrs. Probert and Soward for their joint interesting paper.

The PRESIDENT: In opening the discussion on the papers, I may say that Mr. Bidwell has asked some very pertinent questions, and no doubt, from the number present, there are many gentlemen here who will be able to answer them satisfactorily, and favour us with their views on this interesting subject.

Mr. A. **Stroh**: Mr. Bidwell having referred in his paper to my experiment showing the repellent action at the microphonic contact, which I described at our last meeting, I wish to say a few words in answer to his remarks.

At first, he said, in carrying out a similar experiment to mine, he obtained the same results as I did; but after a time he found that, when he placed a small weight upon the loose carbon, the deflection he obtained was almost the same as before, while the microphone failed in recording sound-waves. My answer to this is, simply, that the microphonic condition still existed when the little weight was placed on the loose carbon; and, if Mr. Bidwell had employed stronger sounds than the ticking of a watch, I believe the microphone would have still acted as such.

Mr. Bidwell's paper deals with a very difficult subject, and the amount of work it represents is considerable. His experiments are certainly highly interesting, and, as I can bear out some of the facts he obtained, I will give a short account of some experiments I made since our last meeting.

The microphone I used was again of the hammer-and-anvil pattern. This form is most suitable for experiment.

On a little board, A, Fig. 1, was fixed, by means of a brass holder, *a*, a thin carbon rod, *b*. Against the end of the latter rested loosely another carbon rod, *c*, which was mounted on a little spindle, *d*, and a small spiral spring, *e*, served to vary the pressure at the contact between the two carbons *b* and *c*.

The object of this arrangement was that the points of contact might be brought under a microscope, and for that reason the carbons, where they came into contact with each other, were made as flat as possible, and also thin at the edges, so that they might be fairly brought into the focus of the microscope.

The little board A, on which was placed a loud-ticking watch as a source of sound, was fixed at B on a stand apart from that of the microscope, so that touching and adjusting the latter might not interfere with the microphone. A hole in the board at *h*, under the contact, is necessary to admit light from below.

In the circuit with the microphone was a telephone, a make-and-break key, and a small battery.

Looking through the microscope, the edges of the carbon contact had a jagged appearance, as represented in Fig. 2., and only one or two projecting points were seen to touch. The tension of the spring for the first observation was such as I knew by experience to be necessary to insure microphonic contact. By depressing the key a current was now sent through, and I observed the effect closely by means of the microscope. What I noticed was a light, or small arc, or, what I afterwards concluded it was, a burning away of the carbon. The two carbons came closer together during that burning, and presently another contact was made, and another light became visible. The carbons still approached until I saw three or four points of contact, each of

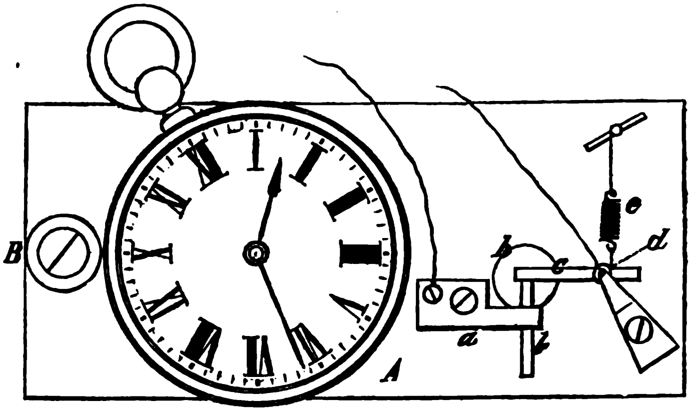

Fig. 1.

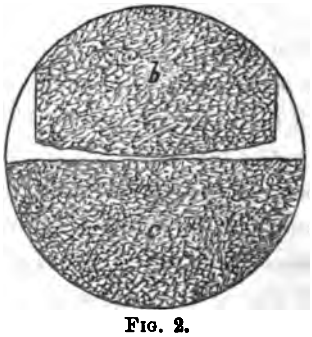

Fig. 2.

which was illuminated; and at last, when, as I presumed, there were sufficient points of contact for the greater portion of the current to pass, the burning ceased.

The PRESIDENT: What battery power had you?

Mr. STROH: Two small bichromate cells.

When the burning ceased I heard the watch tick.

This all took place in the course of a few seconds. While the microphonic condition remained, I could see no longer any light at the points of contact. But as soon as the battery power was increased the burning commenced again, and continued until the area of contact was sufficiently enlarged, and, consequently, the resistance so far reduced that the degree of heat which was still produced was below that which causes the burning of the carbon.

This, I believe, agrees entirely with Mr. Bidwell's observation, viz., that the resistance of the contact falls with an increase of current.

But this was not the only thing I observed. I now let down the spring of the microphone, so that the two carbons were only just in contact, and no more. I then passed a current from the two cells, and saw a momentary burning and heard a short click in the telephone; then followed an interruption, and the current would pass no more. By making contact now with the key many times, no trace of current could be detected with the telephone, showing that after the first passage of the current great resistance was established. This is also an effect observed and investigated by Mr. Bidwell.

I now screwed up the tension spring a little, and so increased the pressure: the insulating film or substance which prevented the current from passing was by the extra pressure forced away or broken through, and another current passed on making contact, but only for a moment, and then again the interruption occurred. This I could repeat several times, until the spring was wound so tight that the insulating substance was broken through as soon as it was formed, and then I could hear in the telephone the well-known boiling or hissing sound which we hear when the microphone is out of adjustment. Sometimes the noise was a perfectly clear musical note, and by adjustment of the spring it occasionally became as shrill as a railway whistle.

I have been able to adjust the microphone so, that while still listening to the whistling noise, I could hear the watch as well; and, whenever I have succeeded in doing that, the tick of the watch was always louder than I have obtained it under any other circumstances.

That experiment was made with a telephone which had a considerable resistance in the coil. I then took a telephone with a very small resistance, only a few ohms, and obtained an entirely different effect. I let down the spring again, so that the carbons were only just in contact. I saw a momentary burning as before, but the movable carbon was driven away from the fixed carbon, and apparently remained repelled at some distance. Each time the circuit was completed by the key, the movable carbon was driven away, and I could see a continuous stream of sparks flying across the space. The sound produced in the telephone by this effect was like a miniature artillery bombardment. By tightening the spring the noise was changed into a musical note, the pitch of which rose with the pressure.

By close observation I could see very fine dark lines, which reached from every projection of the jagged edge of the loose carbon, right across the space to the fixed carbon. This showed that the movable or loose carbon was in a state of vibration, the amplitude of which was equal to the distance of the apparent separation of the two carbons. This distance, which was considerable when the rate of vibration was low, became rapidly less as the rate was increased by tightening the tension spring, and a point was soon reached when the separation could be no longer seen, but was likely still to exist during the production of the higher notes.

Another effect which I have occasionally observed with the microscope during the production of the various noises above mentioned was, that little fragments of carbon, which probably became detached by their expansion by heat, but still remained between the carbons, were in a state of agitation.

There can be no doubt that during the production of these singing, hissing, or boiling noises the current cannot be a constant one; but in the case of the former it must be intermitted, and of equal periods, while in the case of the latter it is probably undulatory, and of unequal or irregular periods.

It also appears to me that a current crossing a microphonic contact has a strong tendency to cause these vibratory disturbances, and I am inclined to believe that during microphonic action, even when all is in good adjustment, the sound-waves which are transmitted are accompanied by other vibrations which are due to the passage of the current itself.

The following reasoning has led me to this supposition:—Whenever I succeeded to adjust the microphone so that I could hear the watch tick during the production of singing or hissing noises, the timbre or quality of the sound of the ticking was exactly that of the hissing, and any change in the character of the latter was always accompanied by a corresponding change of the former.

As it is a fact that sudden small changes in the quality of sound are sometimes observed even when the adjustment of the microphone is perfect, that is to say, when no singing or hissing noises are heard, it must be that whenever sound waves are transmitted these have superimposed upon them other vibrations possibly of a very high rate or pitch, which vary and so produce change of timbre or quality. The latter would be the vibrations or disturbances produced by the passage of the current across the contact.

I next substituted metallic contacts for the carbon ones. With platinum I have obtained very good microphonic effects whenever sticking could be avoided, and it occurred to me during an experiment to place a little oil on the platinum contacts. The oil by its capillary attraction remains always surrounding the contact, and, when the adjustment was so that I could hear the watch tick, I saw that the oil was in violent agitation, and little particles of dust or carbon which were in it I could see spin round with great rapidity. I was puzzled for a long time as to what caused the particles to be agitated in so violent a manner, but found that it was simply the effect of heat. The oil was boiling at the point of contact, even with a simple cell, and when I saw an exceedingly thin film of smoke arising from near the contact I was convinced that such was the case.

I then placed upon the surface of the oil a number of carbon particles or dust. When the current was again sent through, the whole of the carbon dust was violently rotated and started in all directions, and at last it accumulated round the points of contact and built up a pillar which forced the electrodes wide apart, and took the shape shown in Fig. 3.

When the pillar began to form, it appeared that the carbon dust and particles were attracted to the points of contact by some means or other, and that some, having once got there, could not get away, and one was built upon the other, and so the pillar was formed in the oil.

With a pair of steel contacts I made the same experiments with the oil, with equal success. A curious phenomenon which I especially observed with steel contacts was, however, that the revolutions of the black particles in the oil would continue their movements for a considerable time after the current had been interrupted by the key. In some cases it took quite two minutes before they came entirely to rest.

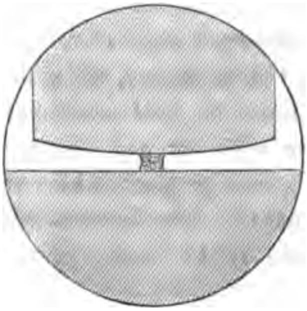

FIG. 3.

Finding that a pillar was also formed without carbon dust in the oil, I took the latter away altogether, and found that by a little humouring I could cause this pillar to grow even when the contacts were dry and clean.

This pillar, while in course of formation, had the appearance of being in a state of low incandescence, but I came to the conclusion afterwards that it was surrounded by a dull red glow, which appeared to be really a succession of sparks running along the surface of the pillar.

I do not pretend to say that the formation of this pillar can have anything to do with microphonic action, but I think it right for us to consider all these little effects, and then, by collecting and compiling them, help some day to explain microphonic action.

At present we are not unanimous. Mr. Shelford Bidwell is in favour of variable mechanical pressure caused by sound-waves as being the principal agency. Mr. Preece is in favour of heat, and others favour the arc and other theories; but, though I can see the possibility that all these theories may take a fair share in explaining the mysterious action, I will be content with simply recording facts, and with the expression of a hope that the day may not be far removed when we shall understand the action of the microphone so completely that we can all join in the same opinion.

The PRESIDENT: As it is rather late, and we have a ballot this evening, I propose to adjourn this important discussion. The paper read to-night by Mr. Bidwell was itself important, and now Mr. Stroh has brought forward fresh points of considerable interest, which I think we shall do well to sleep on.

A ballot then took place, at which the following were elected:—

As Associates:

Burrows, Henry Alpha.
Day, Russell Camper, B.A., Oxon.
Hinton, W. G.
Lanyon, Colonel Sir W. Owen, K.C.M.G., C.B.
Legg, W. A.
Macartney, H. J. Ellison.
Mondey, William.
Pidgeon, Randall.
Sealy, G. W.
Smart, Edgar.
Smith, Thos.
Wharton, Charles Joseph.

As Students:

Joyce, Samuel, jun.
Porter, Charles V.
Rider, John Hall.
Taylor, William.

The meeting then adjourned until Thursday evening, 26th April, 1883.

The One Hundred and Twenty-second Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 26th, 1883—Mr. WILLOUGHBY SMITH, President, in the Chair.

The minutes of the last meeting were read and confirmed, and the names of new candidates were announced and suspended.

The PRESIDENT: Before we renew the discussion on the three papers referring to microphones, I will ask the Secretary to read a communication received from Professor Silvanus Thompson, giving his views on the subject.

The SECRETARY then read the following communication forwarded by Prof. Silvanus P. Thompson, who was unable to be present:—

In the first place, I cannot agree with Mr. Bidwell, that the variation of contact resistance by pressure was first discovered either by Count du Moncel or by M. Clérac. It is much older. It was to obviate imperfect contacts and their undesirable variations of resistance, that binding screws—more than half a century old—were devised. And it is amongst the first things impressed on the attention of young telegraphists, that they must in signalling press their keys well down, because a light contact would offer some resistance, which on an increase of pressure would disappear.

In the second place, I should like to draw attention to two matters, the importance of which, I think, has been overlooked too much both by Mr. Bidwell and by Messrs. Munro and Warwick. These two points are—

1. *The range* through which the *resistance* of a contact can vary.

2. *The range* through which the *distance* of a contact can vary. (The sense in which I use this term will be evident presently.)

According to my experience, the *range of resistance* through which the contact between two conductors (including both metallic and non-metallic conductors) can vary is very wide.

With two pieces of silver or copper the range is enormously wide, from a very small fraction of one ohm up to some thousands of ohms, without any break. I find that the available range of variations in resistance is not so great when materials of lower conductivity are used. For instance, with bad conductors, such as selenium, whose conductivity is millions of times less than that of copper, silver, graphite, or any ordinary good conductor, it is impossible to get equally wide ranges of variation in resistance. For though the higher limit may be as great, no amount of pressure at a point will bring the bad conductors into a sufficient degree of intimacy of contact to bring down the lower limit to equality with the lower limit of the good conductor.

On the other hand, if we measure *the range of distance* through which the contact-pieces, or either of them, must be moved in order to pass through the entire available range of variations in resistance, the relative status of the different substances is precisely reversed. The best conductors require only the very smallest range of motion, and the worst conductors require a much greater. Of course, in any case the range is exceedingly small—to be measured in millionths of an inch. But I have satisfied myself that it is less for contacts of silver, copper, or brass, than for nickel or platinum; less for platinum than for coke-carbon; and less for coke-carbon than for peroxide of manganese. I should like to enquire whether Mr. Bidwell has taken any cognisance of this point in contrasting the behaviour of bismuth with that of coke-carbon.

I think it is to be extremely regretted that Mr. Bidwell has not extended his very valuable observations to metals more typical in their character than bismuth, which, being the most fusible and worst conductor of the metals (save quicksilver), is not only the least typical of metals, but is the one metal least suited to form a telephonic transmitter. For this reason I must respectfully submit, that all general conclusions drawn with respect to "the metals," in contrast with carbon, must be distrusted *ab initio*, when the one metal selected for comparison is the very one that is least suitable for selection.

For some months I have been watching the behaviour of some metal transmitters devised by Mr. G. L. Anders, and some devised by myself. I hope on a future occasion to bring these instruments before the Society. But it will not, I think, be premature to say, that amongst these instruments—working by contact between lumps of polished metal—there are some which have been in constant action for five or six months, and show no sign of deterioration or of uncertainty in action. They are louder, and in some respects more sensitive, than any carbon transmitter; and they will stand a stronger current. Their articulation, though in some respects a little "hard" or "metallic," is in general more distinct than with carbon contacts. The only real difficulty we have found in arranging them at first has been to secure the requisite *minuteness* in range of motion. Where this is properly ensured, articulation is always good. There is a marked difference between the metals for practical purposes; and on this point much might be said. Suffice it to remark, that with contact-pieces of silver the articulation is loud and clear, and the higher the polish of the surfaces, the better the result. I attribute the superiority of these metal-contacts over carbon-contacts in the matter of loudness to the superior conductivity of the metals employed, giving (with a narrower range of motion) a greater range in the available variations of resistance.

It is the opinion both of Mr. Anders and of myself that the resistance of a contact for telephonic purposes (I am speaking here only of a single contact between two pieces or parts) is determined solely by the number of molecules in contact at the surface and by the specific conductivity of those molecules. The element of fusibility comes in to spoil the constancy of the surfaces in contact; and the elevation of temperature occurring at the contact may, as Mr. Bidwell has well observed, come in to change the specific conductivity of the active molecules. But granting this effect of heating, there remains the implied preliminary fact, that the number of molecules, or, in other words, the amount of substance in contact, will be varied by changes of pressure. At a mere "point" in contact physically with another "point," there may be hundreds or even millions of molecules in contact with one another, all acting as so many paths for the flow of the current. An extremely minute motion of approach or recession may suffice to alter very greatly the number of molecules in contact, and the higher the specific conductivity of the substance, and the denser its structure, the shorter need be the actual range of motion requisite to bring about a given change in the contact-resistance. Just as in a system of electric lamps in parallel arc, the resistance of the system increases when the number of lamps is diminished, and diminishes when the number of lamps connecting the parallel mains is increased, so is it with the molecules at the two surfaces of contact. Each molecule as it makes contact with a molecule on the opposite surface diminishes, by so much relatively to the number of molecules previously in contact, the resistance between the surfaces. Each molecule as it breaks from contact with its opposite neighbour adds to the resistance between the contact-surfaces. It may therefore be that the variations of resistance which are observed at contacts between all conductors, from the best to the worst, are all made up (though they *appear* to pass through gradual and continuous changes) of innumerable minute "makes and breaks" of molecular contact.

Assuming then, broadly, that the observed resistance at a point of contact is due to the number of molecules in contact, and to their individual resistances, it is evident that the property of varying resistance at contact ought to be most evident *ceteris paribus* in those substances which are the best conductors. Unfortunately, the *cetera* are not *paria*; owing to questions of unequal fusibility, porosity; and carbon, which is so remarkably infusible, is commonly credited with giving a better result than other substances. This common opinion is, however, based on comparisons made without taking into consideration the question of range of motion, and without taking into consideration the point that some carbon won't work at all in a microphone, namely, that kind that does not conduct, whilst that kind works *best* which is the *best conductor*. In fact, in a telephone transmitter, properly arranged with a suitable range of motion in the parts, the metals work just as well as carbon—some better. As to the suggestion that the alleged superiority of carbon is due to its being a "semi-conductor," whatever that term may mean, that suggestion is one of those peculiarly fitted to catch the unscientific mind, as affording an easy explanation for an obscure fact; and, like a good many other catching suggestions, it is not true. Good conducting carbon works well: bad conducting carbon works badly; and silver may be made to work better than either—at any rate, so long as the surfaces do not get altered by partial fusion.

Lastly, a word on the use of metallic transmitters. I cannot admit that Messrs. Munro and Warwick—interesting and valuable as their new instruments are—are the first to use metallic transmitters. Messrs. Theiler have, to my knowledge, long employed metallic transmitters. But before them Blake certainly deserves the credit of knowing that metallic contacts could be employed. Blake's British Patent Specification for his well-known transmitter distinctly mentions this, and he does not even claim the use of the carbon stud. Speaking of the metal weight attached to the spring, he says: "This weight may be of metal, which may serve directly as the electrode."

But older even than Blake there are metal transmitters: Philipp Reis employed them. I think Mr. Munro said that Reis employed, "without knowing it," a metal microphone. I quite admit that he did not know the *term* microphone; but careful perusal of his works leaves no doubt that he used and meant to use the thing. That he did not intend his instrument to work as a "make and break," is obvious—firstly, from the fact that, like Blake, he mounted his contacts on following-springs, which could have no other result than to prevent abrupt make and break; secondly, he himself has told us that the contact-apparatus which he put at the back of his tympanum was intended to open and close the circuit in proportion to the vibrations. No carbon microphone can do anything more than this, though the quaintness of the way of putting it may evoke a smile.

The **President**: I am anxious that the discussion should proceed without delay, as there are one or two instruments to be shown this evening. Professor A. Jamieson has come specially from Glasgow to introduce to our notice a new meter, the invention of Professor Blyth. Another instrument before us, is a static induction telephone by Mr. Moon, and we should be glad to have these instruments shown to-night.

Professor D. E. HUGHES, F.R.S., in opening the discussion, said there were two points to be considered in connection with microphones, one being the mechanical condition, and the other the electrical condition, the two points being entirely distinct. Dr. Julian Ochorwiz, he said, experimented in 1879,* and showed some very interesting experiments in regard to the theory of the microphone. He (Professor Hughes) had himself attempted to show that a microphone would work without a diaphragm, and he showed a sketch of an arrangement of carbons which dispensed with the mechanical motion of a diaphragm, and by which speech was transmitted in spite of the presence of the grating noise. A microphone suspended in a glass vessel filled with water gave out sounds quite clearly when the water was spoken to, or when the glass was spoken to, showing that mechanical motion took place at every point. He expressed the opinion some time ago, and still adhered to it, that there were two kinds of vibration, acoustical and mechanical. If a log of wood was struck at one end, the sound was heard perfectly at the other: that was mechanical vibration, which took place in every part of the wood. Sir Charles Wheatstone, in his earliest telephone, adopted this fact in his transmission of musical sounds from one room to another in the Polytechnic Institute, by the employment of a single rod of wood. But no transversal vibrations took place in such an arrangement; every portion of the wood was set in vibration, as in the sounding post of a violin; and the transmission was really by molecular action.

A microphone placed on a piece of wood which was set in mechanical vibration would be influenced by the motion: if a hole were made in the wood, the sound would be still heard, or if the hole were filled with water, the effect would be as clear and perfect as in the first instance. An ordinary microphone, coated thoroughly with sealing-wax and made quite rigid, would act perfectly well for some time, until a burning or disintegration of the particles took place, when the microphonic action ceased.

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* *Telegraphic Journal*, November 1, 1879.

In 1878 he read a paper before the Physical Society, in which he took the ground that mechanical vibrations were quite distinct from acoustical vibrations, and could not be considered as taking place in large waves: there was no condensation on the one side and expansion on the other. To him, at first, it appeared that there was no other way of explaining the electric action of the microphone than the theory that the carbon surfaces came more or less into contact by molecular action. The theory was a most natural one, though many anomalies occurred that it did not account for. Mr. Bidwell, in his paper, had taken the same view; but that paper was so full of anomalies that, in fact, nothing could disagree better with his theory than his own paper. No better conviction is needed if Mr. Bidwell had a theory, but, fortunately, he said he had not. Mr. Bidwell and Mr. Stroh, as well as others, had shown that something remarkable occurred in the action of the microphone. He had made many experiments to try and find out what that something was, and came to the conclusion that it was, as in the electric light, the effect of an arc. A spark occurred, however low a battery power was used; and even when it was not visible, there was no doubt a theoretical spark; and an arc supplied all the conditions to account for the action. M. Ader had experimented in Paris with a microphone in which an arc of some size took place. The carbons were arranged at a distance of a quarter of an inch, through which the arc was formed, and through which speech was perfectly transmitted; though, at the same time, not without the great noise of an electric arc, on account of the transference of carbon which took place. Professor Blyth had similarly experimented. Professor Hughes himself was quite ready to adopt that view, but there were many anomalies even after that. He and Dr. Warren De la Rue had experimented with the view of measuring the arc with varying battery power, and with the terminals placed in a tube at an infinitely small distance apart, within one-thousandth of an inch. Ten elements were first employed, and a measurement taken on a galvanometer. A deflection took place, which remained constant for a short time, and then the current ceased passing. Twenty elements were next employed, but no current was observed; and it was not until fifty elements were brought into circuit that a current passed, but only for a few seconds. This phenomenon occurred with each increase of current up to 10,000 elements, the current refusing to pass through $\frac{1}{10}$ of a millimètre *in vacuo*, whilst the same current had a striking distance in the atmosphere of $\frac{1}{4}$ inch. Dr. De la Rue, who had made a great number of experiments with tubes, could not explain the effect, and was continuing his experiments in search of an explanation. The action of the microphone was something which took place in a space between two conductors infinitely near, but still separated. It is known as the microphonic contact, and is really electricity in a free space. When we become able to solve the problem of the microphonic contact, we shall add greatly to our knowledge of electricity itself.

Professor W. E. Ayrton, F.R.S., remarked that the title of the paper on "Microphonic Contacts" was, in his humble opinion, very much like the title of a well-known classical treatise on "Snakes in Iceland," which, after stating the title, gave as the substance of the treatise: "There are no snakes in Iceland," and passed on to something else. "Microphonic Contacts" could be similarly treated: there were no microphonic contacts, and in his opinion the action was not at all an action of contact. "Microphonic Separations" might well, he thought, have been the subject of the paper. The point dealt with by Professor Silvanus Thompson, viz., that of range, was in fact another reason for making it fairly certain that there was no actual contact at all in a microphone. Professor Perry and the speaker had made experiments on the electro-motive force that would maintain an electric arc at various distances between the carbons, and a communication had been made by them on the subject to the Physical Society at the end of last year, which would appear in the "Philosophical Magazine" for May. The law that was evolved was that, given a certain distance between two carbon points, there was a certain fixed electro-motive force that would keep up the arc: less than that would not keep up an arc at all, and one could not make the electro-motive force greater than that, because the only effect would be that more current passed—in fact, that the potential difference necessary between two carbon points to maintain an arc depended mainly on the length of the arc, and hardly at all on the current passing. An investigation of the experiments of Drs. Hugo Müller and De la Rue on the subject of the striking distance of arcs showed that the curve recording Professors Ayrton and Perry's results, connecting the length of an arc with the potential difference between the carbons necessary to maintain the arc, agreed very closely with the curve Drs. Hugo Müller and De la Rue had found for potential difference and striking distance. Further, an examination of those gentlemen's experiments showed no recorded results on the potential difference between two carbon points when an arc was maintained, but only the potential difference between two brass or metallic points; and it was surprising to find that it required for a given distance an immensely greater electro-motive force to keep up an arc between two metal points than one between two carbon points. Professor Silvanus Thompson's remarks coincided with that, for he said that in a metallic microphone the range was very much less than in a carbon microphone, and this Professor Ayrton pointed out meant the maximum electro-motive force of the battery was only able to maintain a current across a very small distance separating two metallic surfaces.

The experiments of Drs. Müller and De la Rue, compared with those of Professors Ayrton and Perry, showed that the potential difference necessary to make an arc of fixed length between two metallic points was very much greater than was necessary with two carbon points. It had occurred to them, therefore, as probable that with two very soft pieces of carbon, such as that proposed by Mr. Varley for the electric light, an arc would be able to be maintained with very much less electro-motive force for the fixed distance than was supposed; and he was glad to see that the result they had then predicted had been since experimentally verified with Mr. Varley's very soft carbons, it having been experimentally found that with soft carbon a very much less difference of potentials was necessary to keep up the arc of fixed length than would be necessary with hard carbon. It seemed that for a given substance at a fixed distance a certain potential difference could be maintained, and practically no other, the difference of potential itself being dependent on the substance and on the distance.

He had spoken of no real contact at all up to that point. It would be observed that in Mr. Bidwell's paper, so long as he dealt with a very small pressure, such as $\frac{5}{100}$ of a gramme, a slight increase in the pressure made a considerable difference in the resistance, but when he got to 3 grammes the additional pressure of, say, ½ gramme made only a small percentage of diminution in the resistance, because then he was approaching real contact between the pieces of carbon, and had ceased to have a microphonic—he would not say contact—but a microphonic non-contact. The figures contained in the paper supported the suggestion that there was no real contact in the microphone. In his paper Mr. Bidwell had shown that the resistance diminishes as the current increased. That was exactly in accordance with the results Professor Ayrton and his colleague had found with the arc. The result of Mr. Bidwell's experiments was shown in his Table 5, containing results obtained with a fixed pressure and a variable current, and he obtained by an increase of current a very considerable diminution in what was called the resistance. In Table 9 it was a little more difficult to explain the law. First, there was a diminution of resistance, as the speaker would have expected, but then, as the current increased, the resistance suddenly became infinite. The only explanation, as far as he could then see, was that when the current reached a certain value for a particular pressure the tendency of the particles to be driven apart by the current took place so rapidly that practically no current could pass. What he meant by that was, that when the pressure was greater than a certain amount for a particular current, the pressure was more effective than the repelling action of the current itself between the surfaces, and the current passed; but when a certain current was reached, then the repulsion balanced, or more than balanced, the effect of the weight. When the current stopped, probably the particles came together, but the moment the current was caused to pass, the electro-motive force used being large, a tolerably strong current probably did pass, but for an exceedingly short time, and the particles were at once pushed asunder, and the circuit was interrupted. Every time a strong current was put on, the particles were driven asunder, and, as far as the galvanometer could indicate, no current passed. He anticipated that the strong current did pass, but for so short a time that no effect on the galvanometer was produced. If this explanation, which he ventured to propose, were correct, it was not surprising that Mr. Bidwell found the weak current to pass, because the electromotive force was not sufficiently strong to repel the particles. It seemed an anomaly to say that a weak current would pass while a strong one would not, but it was explained by the repellent force produced in the particles by variation in the strength of the electro-motive force.

All the phenomena mentioned in Mr. Bidwell's paper seemed to him to lead to the conclusion expressed by Professor Hughes, that a great part, if not the whole, of the action of a microphone was due to the particles not being in contact when the current passed. But what had to be dealt with was, in fact, not metallic conductivity, but rather an arc conductivity, or a conductivity in which the resistance was a function of the current, and in which the resistance diminished as the current increased.

Mr. J. MUNRO: I have some further observations to make on the action of the microphone, but, before doing so, I should like to remark upon a point raised by Mr. Bidwell at the last meeting of the Society, in drawing some deductions from his very interesting experiments on "microphonic contacts." Mr. Bidwell has compared a microphone made of a single pair of carbon points with one made of a single pair of metal points, and, because he finds that the metal points tend to stick together, he infers that carbon is superior to metals in the microphone for this reason. Now, it is quite true that a single pair of metal points do tend to adhere, and I believe Mr. Bidwell is correct in arguing that this is one reason at least why metal microphones with single points have hitherto failed. But, as a matter of fact, metal only requires a different treatment from carbon to make it answer well enough as a microphone transmitter. Attempts to use it have failed until now, because it was employed like carbon in the original forms of microphone devised by Professor Hughes. The experiments I have made show that the proper way to employ metals in the microphone is (1) to have a number of contact points, so as to reduce the tendency to adhere; (2) to deal with light masses of metal capable of vibrating easily; and (3) to reduce the vibrations of the sound-waves to a size which does not cause the metal points to separate beyond the discharging distance between them. This distance appears to be smaller in the case of metals than in the case of carbon, probably because carbon supplies more particles to the point-discharge. I use this term in preference to "arc," because I hope to show that it is not an "arc," as we understand it, which exists in a good working microphone, but rather a silent or steady discharge between the points without the production of light. If these conditions be borne in mind, I see no reason why metal microphones should not act as well, practically speaking, as carbon. Metals have even some points of superiority over carbon. They are less resisting in their mass, and, as it is now proved that all the change of resistance which takes place in the microphone is at the points in contact, this is some advantage. Again, they appear to be more capable of very delicate and rapid vibration than carbon. The gauze and grain transmitters with multiple points of contact and light vibrating masses, which I exhibited here on March 8th, are constructed on the lines laid down, and Mr. Warwick has since made two for me in which the gauze and grain microphones are arranged in series—that is to say, several gauze microphones are connected in series in the same case, and several grain microphones are connected in series in another case. This plan will further defeat the tendency to stick, and maintain a uniform and distinct sound; for if one, or even two, of the regulators should stick at once, the current will be short-circuited through them, and the others will continue to act. A transmitter of this kind is placed upon the table to-night. Metals have their own peculiarities, and require a special mode of manipulation, which has not hitherto been recognised.

I come now to the action of the microphone in general. I have been trying to get clear ideas on the subject, and have made a number of experiments to this end, which I will describe as I go on. The view I put forward at the meeting of March 8th still seems to me correct, in that there is a discharge between the vibrating points through a thin stratum of partially conductive air or liquid, which is varied by the vibrations of the points; but a new element has been introduced, in the form of a repulsion between the points caused by the discharge. This repulsion acts as a kind of "buffer" to the vibrating points, tending to keep them apart when they come together by vibration, and it is this which makes the microphone reversible and capable of use as a telephonic receiver, for it appears to vary with the strength of current. That metal microphones also act as receivers, is demonstrated by the wire-gauze receiver which I have had constructed and placed upon the table. It consists of a pile of small squares of wire-gauze enclosed in a case between two electrodes, which convey the current through it. The head of the elastic pile of gauze bears upon the centre of a mica diaphragm through a small plug of cork, so that, when the pile expands and contracts under the passage of the current, the mica diaphragm vibrates. A spring bearing on the outer surface of the diaphragm, and a magnet pole applied to the other end of the pile, are applied for adjustment. The sonorous effect is very feeble with the instrument exhibited, but it is unmistakable, and serves to demonstrate the point in question. Metal microphones, as well as carbon ones, are reversible.

To descend into detail: if you form a circuit with a battery, a telephone, and two electrodes either of metal wires or small carbon rods, then, on bringing the electrodes into contact, you hear first a click in the telephone, then a buzzing sound, and if you press the points more firmly together you get silence. This is invariably the case. The click is, no doubt, the establishment of the discharge between the points; the buzzing is produced by the vibration of the points in light contact, and the silence by a fixed metal contact operating as if it were one solid piece. The buzzing sound indicates what I may call the "microphonic state;" for I have always found that a metal or carbon transmitter becomes active when this buzzing sound arises. The limits of this microphonic state, then, are, on the one hand, the width between the points which gives no current at all, or, in other words, the extreme limit of the discharging distance, and, on the other hand, the fixity of contact which prevents the points vibrating. In short, the microphonic range lies within the discharging distance through the air, liquid, or other fluid conductor between the points; and we cannot have a microphone unless the points are free to vibrate lightly within this range. This explains what we mean by saying that a microphone is a "phenomenon of loose contact."

On a larger scale, if you take an arc electric lamp with large points and a powerful current (I used 40 Bunsen cells and an arc lamp invented by Mr. Hind), you have the same effects produced, but more visibly. On establishing the arc you get a loud click,—a terrific click, in fact,—and then the buzzing sound, which, however, in this case, may more fitly be described as roaring. On pressing the points hard together, you get silence. Whenever there is a small arc the lamp acts as a microphone of great power; but (although it is reported that other observers have done so) I was not able to get a wide arc with the carbons well apart to act as a microphone. This was not altogether due to the roaring of the discharge smothering the other sound, for sometimes when the lamp burned steadily the roaring fell off. In fact, the roaring was only violent when the lamp flickered and the arc played about; and I have come to the conclusion that the rattling and buzzing in a badly adjusted microphone is owing to a change in the discharge, due to breaking away of the points and a shifting of its ground on the part of the discharge. Carbon microphones are peculiarly liable to this defect, and hence it is, I think, that the platinum contact in the Blake transmitter is advantageous by directing the discharge, as it were, and keeping it in position.

In brief, I found with the arc lamp that it became a microphone when there was a small arc between the points, and the points were free to vibrate on each other. When the arc was wide and the points visibly separated, I got no effect, although on sensibly hitting one of them so as to change the width of the arc materially, the kick was accompanied by a single sound in the telephone. But it was not a true microphonic sound, and had no vibrant quality. On the other hand, when the points appeared to touch, but had a small arc passing between them, they were sensitive to the lightest touch, and became a true microphone. It did not matter though portions of the red hot points actually touched, provided there was something of an arc passing between the other portions. The arc evidently took the place of the gentler air discharge in the ordinary microphone, and was a more intense form of it.

Thus far, then, the requirements of the microphonic state appear to be two conducting points vibrating in close proximity, and perhaps even touching at intervals, with an electric discharge between them. But other elements enter into the problem—attraction or repulsion of the points due to the electricity. The receiving microphone might act with either attraction or repulsion added; and no doubt there is a static attraction of the points at least before the discharge takes place between them, owing to their difference of potentials. This attraction, as I have observed in the paper read on March 8th, may have something to do with the sticking of contacts investigated by Mr. Stroh. But I think the evidence is in favour of repulsion as an important element in the action of the microphone. Mr. Stroh's experiment with the beam of light reflected from the loose contact of a carbon microphone exhibits this repulsion to the eye, and it may also be seen by lightly suspending two pencils of carbon, A B, point to point, as shown in Fig. 1, and sending a current through them. As soon as the spark passes, the points are driven apart, and as they swing back again they are repelled again, and in this way a rapid vibration of the points is set up, which operates as a make-and-break. The repulsion is obviously due to the visible spark; and when this is reduced to something scarcely visible, the points are still

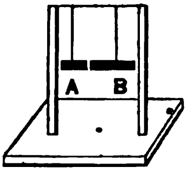


Fig. 1.

seen by the aid of a magnifying glass to be vibrating very rapidly. It seems fair to conclude, then, that this repulsion due to the discharge is active in the microphone.

Mr. Stroh has also seen the carbon points of a microphone in action vibrating under the lens. I have examined metal points with a glass, but failed to notice the vibration, which in the case of metals must be something very small. I have, however, clogged both gauze and grain transmitters with viscous liquids, such as glycerine and vaseline, and in these cases the action of the instrument was very much impaired, and the sounds very imperfectly transmitted, or not at all. Vibration of the points may also be admitted in evidence, I think.

We have, then, two conductive points vibrating in close proximity under the action of the sound-waves, either by direct impact of the waves, or by transmission through solid supports. The discharge taking place between them exerts a repulsive action tending to separate them with a force in some way proportional to the strength of the discharge, and as they vibrate they approach and recede, thus varying the strength of discharge by varying the distance between them. The repulsion acts as a counter spring, resisting approach with a force which appears to increase the nearer they come together, and to diminish as they move apart and the strength of the discharge becomes weaker. This repulsive force accounts for the pressure applied to microphonic contacts. If that pressure be too light, the points tend to swing too far apart, and weaken or break the circuit.

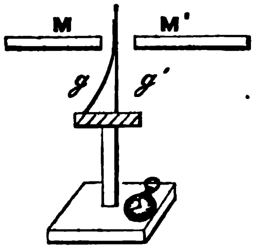


FIG. 2.

In experimenting with a wire gauze transmitter of the kind which has been called the "lyre telephone," and consisting of one movable plate of wire gauze (g, Fig. 2) leaning on another fixed plate, *g*, under slight pressure due to its weight, I always find that, on bringing up the pole of a magnet, M, in front of the movable gauze, I can increase the ticking of a watch transmitted by it. The increase is gradual as the pole approaches, and the sound becomes very loud just before rupture of the circuit by the movable gauze leaving the other. If done carefully, the click heard on breaking the circuit can be repeated by withdrawing the pole a little, and allowing the circuit to be restored by the discharge leaping between the points, which move together again. Sometimes the discharge can be broken so gently in this way, that the final click is barely audible. On the other hand, if the pole be brought up on the other side at M′, the gauzes are pulled together, and the sound of the clicking dies off. It would seem, then, that the lighter the pressure in the case of metals the better. It allows the repulsive force to act, and the points freedom to vibrate within the range of the discharge. The approach and recession of a magnet in this way makes an adjustment to the gauze transmitter far more delicate and gradual than any mechanical device.

The action of the microphone, if the above view is correct, depends upon three conditions—(1) a discharge through a fluid of low conducting power between two conducting points; (2) the sonorous vibrations of the points within the range of the discharge; (3) the repulsion exerted on the points by the discharge. Whether the points actually touch as they vibrate, or barely touch, is perhaps immaterial to the general action. The extent of surface contact does not seem to be an essential either, though doubtless it has a modifying influence on the strength of discharge, just as actual contact would have. In any case, the fluid stratum must be of very small thickness.

The cause of this repulsion is another problem. It may be due to heat swelling the points of contact; but, though heat is certainly produced in the action of the microphone, it seems rather to be an incidental product than an essential in the action. Red hot carbon or metal points act as a microphone. It is difficult to understand how the temperature of the points can vary so rapidly as the microphone requires. The repulsion appears to be due rather to some property of the discharge itself. This may be what Professor Ayrton called "electro-dynamic repulsion," in his remarks on this subject at the meeting on March 8th, or it may be due to expansion of the medium between the points. But through the kindness of Dr. Emmens, who lent me a mercury pump, I have here a "vacuum-microphone," consisting of a gauze transmitter mounted on platinum wires within an exhausted glass bulb, and mounted on a wooden stand. It is remarkably sensitive, and the sounds are peculiarly clear and metallic. The vacuum in this case is about that of an incandescent lamp, and therefore high. Nevertheless, the instrument acts very well, and has the advantage of preventing the gauze from rusting, and adjustment can be made by a magnet external to the bulb. The vacuum, or partial vacuum, still conveys the discharge, as the click can be heard on breaking circuit. It would appear, therefore, that the repulsion is not due to an expansion, but to some phenomenon of discharge not well understood.

Mr. J. J. **Fahie**: It seems to be generally agreed that the active surfaces of the microphone, in its most delicate state of adjustment, are not in actual contact—that there is an intervening air space across which the current is conveyed, but in what manner we cannot say at present. Conceding this point, it seems to me that many of Mr. Bidwell's experiments might be explained on the principles of static induction. Turning to Mr. Bidwell's paper, he says, in reference to Table IV.: "In Table IV. are given the results obtained with constant currents of ·1 and ·001 ampère, the pressure at the point of contact increasing from ·05 gramme to 25 grammes, all of them representing the mean of several series of measurements. It will be seen that with small pressures the resistance is largely dependent on the strength of the current, but when the pressure is considerable the resistance with weak and strong currents is nearly the same."

I would explain this peculiarity somewhat as follows:—Induction precedes conduction. Static charges are collected on the opposing surfaces of the carbons. These charges are proportional to the E.M.F., and, according as they are great or small, the attractive effect will be great or small: the carbons will be pressed together with more or less force, and the result will be that more or less points of contact are established (remembering that carbon is compressible), and, consequently, that the resistance will diminish or increase with an increase or diminution of the E.M.F. Now, the inductive charges from $\frac{1}{1000}$ of an ampère are small, consequently the force with which the carbons are attracted, in order that the charges may combine, is also small: only a few points of contact are established, and the resistance as measured by the galvanometer is great. But, with an E.M.F. of $\frac{1}{10}$ of an ampère, all the effects are intensified 100 times: many more points of carbon are brought into contact, and the resistance diminishes in a corresponding degree. This is what happens when the pressure is small, or $\frac{1}{20}$ of a gramme.

When the pressure is great, or 25 grammes, the effect is less one of action at a distance, or of make-and-break contact, and resembles more nearly ordinary conduction; hence, as shown by Table IV., the resistances ought to be nearly the same with the two E.M.F.'s.

By applying this reasoning to Tables V., VI., and VII. of Mr. Bidwell's paper, the observations therein recorded will, I think, be quite intelligible. Referring to Table VII., Mr. Bidwell says: "It will be noticed that the final resistance is almost invariably slightly higher than the original resistance, but this increased resistance gradually diminished, as may be seen by comparing the last figure of one line with the first of the next, and perhaps, if time were given, it would return to its original value. There is little doubt that this is a thermo-electric effect."

Mr. Bidwell's surmise may be correct, but I would just throw out the hint, that part of the effect may be due to the rebound of the carbons on the removal of the pressure, or, to some alteration of their relative positions brought about in the act of removing the pressure, the carbons gradually working themselves back into positions of greatest efficiency, just as we see a piece of clockwork do, whose bearings have been shifted.

Mr. Bidwell's experiments on the effect of heat on the resistance of carbon contacts are very valuable from my point of view. I am not surprised that he found they did not bear out the assumption that the heat engendered by an increased E.M.F. should reduce the resistance. I say I am not surprised, for I am unable to appreciate the *very small* heating effect of a *small* fraction of a Leclanché cell on a *large* surface of carbon. According to my view of microphonic action, the resistance with a great E.M.F. is reduced, not because of any heating effect which the current may have on the carbons, but because a great E.M.F. is able to charge the opposing surfaces of the carbons to a higher potential, under the influence of which they approach and press together more points, and so offer more roads to the passage of the current, thereby reducing the total resistance. Indeed, Mr. Bidwell himself seems to refer to some such view, when he says a little further on in his paper: "The reduction apparently produced by current in the resistance of the contacts is probably quite unconnected with its heating effect, and may be due in part to increased electro-motive force."

In summing up his experiments on this point, Mr. Bidwell says: "So far as they are worth anything, they tend to show that the resistance of a carbon contact is increased, instead of being diminished, by heat. This may possibly be due to the formation of a non-conducting film by air or gases which the heat expels from the carbon."

Now, on my assumption, a very different explanation, and, to my mind, a very sufficient one, offers itself. Heat rarifies the air stratum which I suppose to separate the carbons; and rarified (or hot) air, as we all know, has a lower inductive capacity than ordinary (or cold) air; therefore the inductive charges on the carbons, and all that follows from them, will be less in proportion as the temperature rises, the result being an increased resistance.

The peculiar buzzing sounds which are sometimes heard in the microphone recall an old experiment of mine which seems to bear on this point, and which led me to take the view I do of microphonic action. I took an ordinary Morse local circuit, consisting of four low-resistance Daniell cells, the electro-magnet of the printing instrument, of about six ohms, and the relay,—a Siemens' polarised one. I first regulated the stops, so as to make the play of the tongue as small as possible, then I adjusted the position of the tongue itself with respect to the poles of the relay, until I produced what is familiarly known as a "chattering of the relay."

Now, I take this chattering to have been produced in the same way as the buzzing in the microphone. It was the sound given out by the tongue in making and breaking the local circuit. The make-and-break effect I imagine to have been produced as follows:—The attractive force of the static charges accumulated on the battery contact-stop, and the opposing side of the tongue was just sufficient to overcome the inertia of the tongue and the very slight bias which it had to rest against the insulated stop: the tongue consequently moved, the static charges combined, and, there being now no longer any attraction, the tongue returned to its first position, only to be again attracted, and so on. Here the effect was probably heightened by the currents induced in the coils of the electro-magnet, whose armature, I may remark *en passant*, was only spasmodically, and often not at all, affected. Such I conceive to be the action of the microphone under the influence of a current: the carbons are first pressed closer together by the attractive force of the static charges, the charges combine, the attraction ceases, and the elasticity of the carbons makes them again separate, only to be again attracted, and so on. The currents induced in the coils of the telephone probably complicate the result, and may be the cause of the sparks which Mr. Stroh saw under the microscope.

Mr. W. M. MORDEY related several experiments he had made with the view of ascertaining the variation of resistance from the passage of strong currents through masses of metallic particles in a state of "loose contact." He took a spherical vessel about nine inches in diameter, which he filled with very fine iron filings. At opposite sides of the vessel two circular copper plates, one inch in diameter, were fixed, and provided outside with binding-screws. The resistance of the arrangement was very high indeed, but very variable, and liable to considerable fluctuation by a mere tap or shake. Its resistance was about 40,000 ohms. The arrangement was switched into the circuit of a series dynamo machine, which had an electro-motive force of about 2,000 volts. The intensity of the arc lamps, also in circuit, immediately fell, but only for an instant. The electro-motive force at the terminals showed a very high deflection—in fact, too high for the meter. A reading taken half a minute later showed an electro-motive force of 500 volts, with a current of nearly 10 ampères,—several of the lamps having been switched out of circuit,—the resistance was therefore about 50 ohms. The current was kept on, and in about five minutes the resistance had fallen to 23 ohms. After a few hours had elapsed, so as to allow the filings to cool, the resistance was found to have gone up again to about 36,000 ohms—still high, but not quite so high as at first. The experiments were then repeated with a dynamo having an electro-motive force of about 800 volts, but it was found that the resistance was too high to allow of the machine "working up" through it. Further experiments were made with an arrangement consisting of a wooden trough an inch deep and two inches wide, filled with very fine filings, as before, and having a copper plate of two square inches at each end. Six cubic inches of these filings had a resistance of 109 ohms, and was, as in the previous case, liable to considerable alteration by tap or vibration. When placed in the circuit of a series dynamo machine having an electro-motive force of 500 volts, and giving under ordinary circumstances a current of about 5 ampères, the lamps in circuit went dull for an instant, but immediately recovered, and the difference of potential proved to be 96 volts, the current 5·03 ampères, and the resistance therefore 19·08 ohms. In one minute the conditions were—electro-motive force, 45·6; current, 5·1; resistance, 9·06. Although the passage of such currents as the above through these arrangements reduced their resistance very rapidly, it was generally necessary to employ a very high electro-motive force to start the reduction.

The effect seemed to be due simply to the expansion of the filings by heat, producing better contact, and therefore lower resistance. The rise in the actual resistance of the metal from the same cause was quite insignificant in comparison, and was much more than counterbalanced by the greater intimacy of contact. Perhaps these experiments throw a little light on the fall of the resistance of carbon on heating.

The **President**: When the microphone was first brought to the notice of this Society, I ventured an opinion that its action was simply due to "loose contact"—that fruitful source of trouble and expense to the practical electrician—and at the present time I still adhere to my first impression.

In the early days of telegraphy it was considered, when testing insulated wires, that quite sufficient connection was made between the "lead" wire from the galvanometer and the wire under test if they were simply hooked together. Improved apparatus soon showed that such contacts were most unreliable, and then binding-screws were adopted; but now, when great accuracy is desired,—for instance, when testing long lengths of submarine cable,—then soldered connections are used.

Many years ago a dispute existed with regard to the electrical condition of a length of submarine cable. It had been tested and pronounced faulty. I then tested it and pronounced it perfect, but when it was afterwards tested in my presence there were certainly indications of something wrong.

No doubt you all know how a submarine cable is tested; many of you here I know do. One pole of a battery is put to "earth" and the other pole connected to one terminal of a mirror reflecting galvanometer, the other terminal being connected to the coil to be tested, which is usually placed in a tank of water. In a few seconds the cable becomes charged to nearly the same potential as the battery, the difference of potential being indicated by the deflection on the galvanometer. With a perfect cable the deflection should very steadily decrease during the test, any sudden throw or kick on the galvanometer indicating that something was wrong—that either the battery or cable potential had suddenly fallen. Now this is what took place with the cable under dispute. The batteries were changed, and the "lead" wires changed, but still at times the throws occurred. At last it was found that the "earth" wire was fastened by a nut to the side of the iron tank, and that the nut had become in course of time slightly loosened, so that every time a puff of wind acted upon the side of the tank, or the water was agitated, then the kicks commenced.

We had here, without knowing it, a microphone; and it was fortunate that the cause of the "kicks" was found, or the Society might have had a paper showing how the wind, or slight agitation of the water, affected the testing of submarine cables.

In investigating the action of the microphone, it should be kept in mind that with clean contacts and high battery power the resistance will always decrease; but with loose and dirty surfaces the resistance will increase with the battery power; and if the surfaces are firmly fixed together by pressure then it matters not whether you use one cell or a thousand, for the resistance will remain the same.

Mr. SHELFORD BIDWELL, in reply, said: The remarks that have been made in reference to my paper were mostly directed to the theory of the subject, and I have not much to say in reply to them. The paper, indeed, consists of little more than a mere statement of facts, of which for the most part I offer no explanation. Professor Silvanus Thompson, in his observations, said that bismuth was the least suitable metal for making a metallic microphone, and in that I have no reason whatever to disagree with him; but my experiments with metals were not made with a view to the construction of the best possible metallic microphone—their object was rather to determine the physical causes of the generally-admitted superiority of carbon over metals for microphonic effects, and these causes would presumably be most evident in the case of metals which made the worst microphones. For this reason alone, therefore, I should have considered bismuth to be the very metal which ought to be selected for the experiments. One thought occurs to me in reference to Professor Ayrton's ingenious explanation of the phenomenon of sudden high resistance. He supposes, in effect, that it is caused by repulsion due to a rapidly intermittent current which continues so long as the circuit is closed or is attempted to be closed. If this is so, there can be no doubt that sounds would be produced in the telephone in circuit; but that is not the case—no sound whatever is heard. I wish I could have selected another title for the paper which would not involve some kind of theory. Of course, the name suggested by Professor Ayrton, "Microphonic Separations," would involve theory quite as much as "Microphonic Contacts."

The PRESIDENT: Gentlemen,—I think you will all agree with me that we should give Mr. Shelford Bidwell a hearty vote of thanks for his instructive and interesting paper.

The vote was heartily carried.

The following paper was then read:—

PROFESSOR BLYTH'S SOLENOID GALVANOMETER, OR AMPÈRE-METER.

By Prof. A. JAMIESON, C.E., F.R.S.E.,
College of Science and Arts, Glasgow.

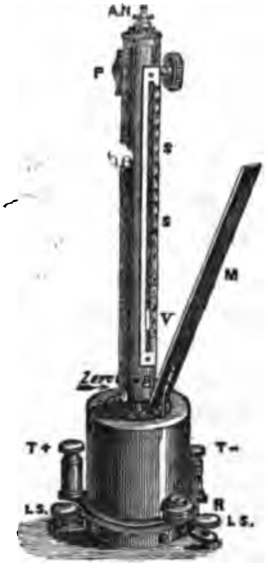


FIG. 1.

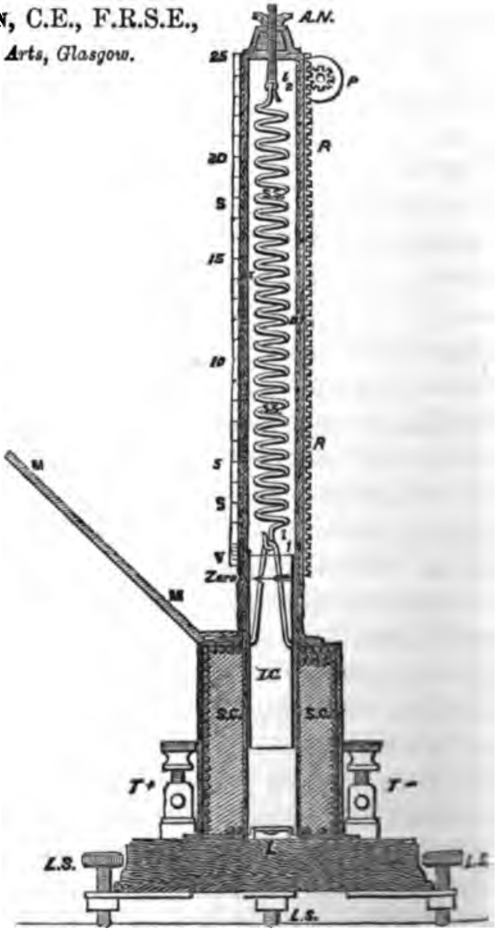


FIG. 2.

The instrument which I have the honour to introduce to you this evening has been invented by Professor Blyth of Anderson's

College, Glasgow. It is called a Solenoid Galvanometer, and is used for measuring the strong currents employed for electric lighting and the transmission of power. Its construction and action are very simple indeed, as will be seen by referring to Figs. 1 and 2 and the following index of parts:—

When T + and T − represent the + and − terminals to which wires from the battery or dynamo are attached. The current passes from T + to T − through

S C	„	Solenoid coil, composed of some three or four turns of insulated copper wire, No. 8 B.W.G., and = 0·07 ohm resistance
I C	„	Iron core, made of best soft iron, in the form of a thin tube. When the current passes, I C is sucked down inside S C, proportionally to the current strength against the resistance offered by
S S	„	Spiral spring, which is attached to I C by loop l_1, and at top by l_2 to
A N	„	Adjusting screw for adjusting the zero of instrument, as marked on core I C.
R and P	„	Rack and pinion fixed to
B T	„	Brass tube, which slides freely inside an outer brass tube.
V	„	A vernier fixed to B T, and which by its position indicates on
S	„	A finely-divided scale (fixed to the outer brass tube), the current strength or number of ampères flowing through the solenoid coil, S C.
M	„	Mirror, by looking down upon which the zero mark on I C (which is to be seen owing to a hole and pointer in the outer brass tube), as well as the position of the vernier on the divided scale, are simultaneously observed and parallax avoided.
L	„	A spirit level.
L S	„	Three levelling screws.

How to take a Test with this Instrument.

1st. Level it by means of the three screws, L S.

2nd. Free the core I C by releasing three set screws not shown, and observe if its zero mark agrees with zero pointer on outer brass tube, at the same time that zero of veriner, V, agrees with the zero of scale. If not, adjust first by A N, and second by R and P.

3rd. Attach leading wires to terminals T + and T —.

4th. Switch on current (when the iron or solenoid core, I C, will be sucked down inside I C).

5th. Raise I C by R and P until zero mark on I C is again opposite zero pointer on brass tube.

6th. Read now the position of vernier, V, on scale S, and refer to the table attached to instrument for the corresponding ampères.

Advantages of this Instrument.

1. It is a zero instrument.

2. It is unaffected by the earth's magnetism, or local magnets and masses of iron.

3. It is in no way dependent upon so-called permanent magnets.

4. It is not practically affected by temperature.

5. It can be roughly handled when once clamped for carrying, without any fear of damage to its parts.

6. Its range and sensibility are very great. The instrument before you will indicate currents from 0·1 to 24 ampères (correct to the $\frac{1}{10}$ of an ampère), and some are to be made with additional coils, so as to indicate from 0·1 to 25 ampères, from 25 to 50, and from 50 to 100, without change of spring and core, by simply turning a switch and bringing one or more coils into circuit.

Professor Blyth has also made provision for constructing potential as well as energy galvanometers on the same principle. The instrument before you was graduated throughout (at each of the 200 millimètres divisions, representing from 0 to 24 ampères), by the electrolysis of water. This method is very accurate if all the necessary precautions are taken.

PLAN OF CONNECTIONS USED IN GRADUATING INSTRUMENT.

Where B	represents	Battery or current generator.
K	„	Key for closing circuit.
V R	„	Variable resistance coil for adjusting the current to the desired strength.

T + and T − represents		+ and − terminals of
S G	„	Solenoid galvanometer.
V	„	Voltameter placed in
$W_1 T_1$	„	Water tank, kept at one temperature.
P	„	Bent pipe for conducting mixed gases (H_2 and O) over to
C C V	„	Cubic centimètre vessel (of. say, 50 to 1,000 cubic centimètres).
$W_2 T_2$	„	Another water tank, kept at one temperature.
t_1 t_2	„	Centigrade thermometers.
S B	„	Standard barometer (in millimètres).

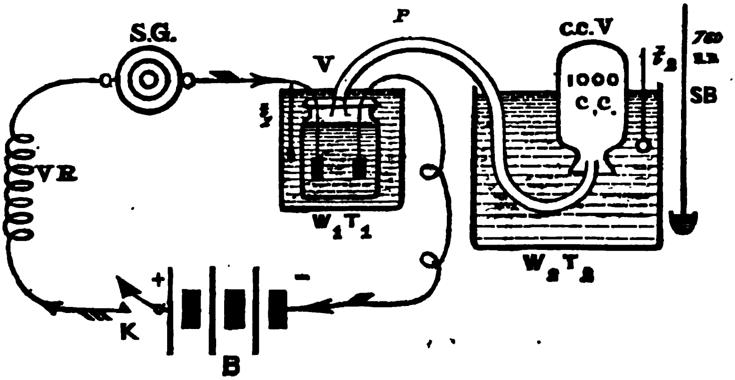


FIG. 3.

The mixed gases are collected by displacement in (C C V) a vessel of known capacity. The time to fill the vessel being very carefully noted by a stop watch, the temperature observed, as well as the barometer in millimètres, we have at once, by a combination of Gay Lussac's and Boyle's laws,

$$C = \frac{V \times 273 \times P}{0{\cdot}1764 \times T\,(273 + \theta) \times 760}$$

Where C = Current in ampères.
V = Volume of gas given off in C C^s.
P = Height of barometer.
T = Time in seconds.
θ = Temperature centigrade of gas evolved.
0·1764 = Volume in cubic centimètres of mixed gas (H_2 and O given off per second by 1 ampère.
273 Constant for variation of volume of a gas with temperature.
760 Standard barometric pressure.

The President: I am sure the best thanks of the Society are due to Professor Jamieson for bringing this instrument before us, and for his description of the same.

A hearty vote of thanks was unanimously passed to Professor Jamieson accordingly.

The following paper, read by Professor Hughes before the Physical Society, in 1878 and published in the Proceedings of that Society, has been handed by him to the Editing Committee, as bearing very closely on the discussion on Mr. Munro and Mr. Bidwell's paper, and it has been considered therefore desirable to republish it:—

ON THE PHYSICAL ACTION OF THE MICROPHONE.

By Professor Hughes.

In the paper read on the 9th of May before the Royal Society, I gave a general outline of the discoveries I had made, the materials used, and the forms of microphone employed in demonstrating important points. I have made a great number of microphones, each for some special purpose, varying in form, mechanical arrangement, and materials. It would require too much time to describe even a few of them; and, as I am anxious in this paper to confine myself to general considerations, I will take it for granted that some of the forms of instrument and the results produced are already known.

The problem which the microphone solves is this—to introduce into an electrical circuit an electrical resistance, which resistance shall vary in exact accord with sonorous vibrations, so as to produce an undulatory current of electricity from a constant source, whose wave-length, height, and form shall be an exact representation of the sonorous waves. In the microphone we have an electric conducting material susceptible of being influenced by sonorous vibrations, and thus we have the first step of the problem.

The second step is one of the highest importance: it is essential that the electrical current flowing be thrown into waves of determinate form by the sole action of the sonorous vibrations. I resolved this by the discovery that when an electric-conducting matter in a divided state, either in the form of powder, filings, or surfaces, is put under a certain slight pressure, far less than that which would produce cohesion, and more than would allow it to be separated by sonorous vibrations, the following state of things occurs:—The molecules at these surfaces being in a comparatively free state, although electrically joined, do of themselves so arrange their form, their number in contact, or their pressure (by increased size or orbit of revolution), that the increase and decrease of electrical resistance of the circuit is altered in a very remarkable manner, so much so as to be almost fabulous.

The problem being solved, it is only necessary to observe certain general considerations to produce an endless variety of microphones, each having a special range of resistance.

The tramp of a fly or the cry of an insect requires little range, but great sensitiveness; and two surfaces, therefore, of chosen materials under a very slight pressure, such as the mere weight of a small superposed conductor, suffice; but it would be unsuitable for a man's voice, as the vibrations would be too powerful, and would, in fact, go so far beyond the legitimate range, that interruptions of contact, amounting to the well-known "make and break," would be prodceed.

A man's voice requires four surfaces of pine charcoal, as is described in my paper to the Royal Society, six of willow charcoal, eight of boxwood, and ten of gas carbon. The effects, however, are far superior with the four of pine than with either the ten of gas carbon, or any other material as yet used. It should be noted that pine wood is the best resonant material we possess, and it preserves its structure and quality when converted into the peculiar charcoal I have discovered and described.

It is not only necessary to vary the number of surfaces and materials in accordance with the range and power of the vibrations, but these surfaces and materials must be put under more or less pressure, in accordance with the force of the sonorous vibrations. Thus, for a man's voice the surfaces must be under a far greater pressure than for the movements of insects. Still the range of useful effect is very great, as the boxes which I have specially arranged for man's voice are still sensitive to the tick of a watch.

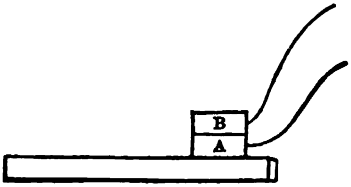
In all cases it should be so arranged that a perfect undulatory current is obtained from the sonorous vibrations of a certain range. Thus, when speaking to a microphone transmitter of human speech, a galvanometer should be placed in the circuit, and, while speaking, the needle should not be deflected, as the waves of + and — electricity are equal, and are too rapid to disturb the needle, which can only indicate a general weakening or strengthening of the current. If the pressure on the materials is not sufficient, we shall have a constant succession of interruptions of contact, and the galvanometer needle will indicate the fact. If the pressure on the materials is gradually increased, the tones will be loud, but wanting in distinctness, the galvanometer indicating interruptions. As the pressure is still increased, the tone becomes clearer, and the galvanometer will be stationary when a maximum of loudness and clearness is attained. If the pressure be further increased, the sounds become weaker though very clear; and, as the pressure is still further augmented, the sounds die out, as if the speaker were talking and walking away at the same time, until a point is arrived at where there is complete silence.

When the microphone is fixed to a resonant board, the lower contact should be fixed to the board, so that the sonorous vibrations act directly on it. The upper contact, where the pressure is applied, should be as free as possible from the influence of the vibrations, except those directly transmitted to it by the surfaces underneath: it (the upper surface) should have its inertia supplemented by that of a balanced weight. This inertia I find necessary to keep the contact unbroken by powerful vibrations. No spring can supply the required inertia; but an adjustable spring may be used to ensure that the comparatively heavy lever shall duly press on the contacts.

The superposed surfaces in contact may be screwed down by an insulated screw passing through them all, thus doing away with the lever and spring; but this arrangement is far more difficult to adjust, and the expansion by heat of the screw causes a varying pressure. It is exceedingly simple, however, easily made, and illustrates the theoretical conditions better than the balanced lever I have adopted in practice.

In order to study the theoretical considerations, and that with the most simple form of microphone freed from all surrounding mechanisms, let us take a flat piece of charcoal 2 millimètres thick and 1 centimètre square, and, after making electrical contact by means of a copper wire on the lower surface, glue that to a small resonant board, or, better for the purpose of observation, to a block of wood 10 centimètres square. Upon this superpose one or more similar blocks of charcoal, the upper surface in communication with a wire, the lowermost surface resting flat, or as nearly so as possible, on the lower block.

The required pressure is put on the upper block; and while in this state the two may be fastened together with glue at the sides, or, better, by an insulated screw. The pressure can then be removed, as the screw or glue equally preserves the force.



Let the lower piece be called A and the upper B. When we subject this board to sonorous vibrations, we cannot imagine an undulatory movement of the actual wave-length in such a mass—that is, a length comparable with the real wave-length of the sonorous wave, which may be several feet. Nor can we imagine a wave of any length without admitting that the force must be transmitted from molecule to molecule throughout the entire length: thus any portion of a wave, of which this block represents a fraction, must be in molecular activity. The lower portion of the charcoal A, being part of the block itself, has this molecular action throughout, transmitting it also to the upper block. How is it that the molecular action at the surfaces of A and B should so vary the conductivity or electrical resistance as to throw it into waves in the exact form of the sonorous vibrations? It cannot be because it throws up the upper portion, making an intermittent current, because the upper portion is fastened to the lower, and the galvanometer does not indicate any interruption of current whatever. It cannot be because the molecules arrange themselves in stratified lines, becoming more or less conductive, as then surfaces would not be required—that is, we should not require discontinuity between the blocks A and B; nor would the upper surface be thrown up if the pressure be removed, as sand is on a vibrating glass. The throwing-up of this upper piece B, when pressure is removed, proves that a blow, pressure, or upheaval of the lower portion takes place: that this takes place there cannot be any doubt, as the surface, considered alone (having no depth), could not bodily quit its mass. In fact, there must have been a movement to a certain depth; and I am inclined to believe, from numerous experiments, that the whole block increases and diminishes in size at all points, in the centre as well as the surface, exactly in accordance with the form of the sonorous wave. Confining our attention, however, to points on A and B, how can this increased molecular size or form produce a change in the electrical waves? This may happen in two ways — first, by increased pressure on the upper surface, due to its enlargement; or, second, the molecules themselves, finding a certain resistance opposed to their upward movement, spread themselves, making innumerable fresh points of contact. Thus an undulatory current would appear to be produced by infinite change in the number of fresh contacts. I am inclined to believe that both actions occur: but the latter seems to me the true explanation; for if the first were alone true, we should have a far greater effect from metal powder, carbon, or some elastic conductor, such as metallised silk, than from gold or other hard unoxidisable matter; but as the best results as regards the human voice were obtained from two surfaces of solid gold, I am inclined to view with more favour the idea that an infinite variety of fresh contacts brought into play by the molecular pressure affords the true explanation. It has the advantage of being supported by the numerous forms of microphone I have constructed, in all of which I can fully trace the effect.

I have been very much struck by the great mechanical force exerted by this uprising of the molecules under sonorous vibrations. With vibrations from a musical box 2 feet in length, I found that one ounce of lead was not sufficient on a surface of contact 1 centimètre square to maintain constant contact; and it was only by removing the musical box to a distance of several feet that I was enabled to preserve continuity of current with a moderate pressure. I have spoken to forty microphones at once, and they all seemed to respond with equal force. Of course there must be a loss of energy in the conversion of molecular vibrations into electrical waves, but it is so small that I have never been able to measure it with the simple appliances at my disposal. I have examined every portion of my room—wood, stone, metal, in fact all parts—and even a piece of india-rubber: all were in molecular movement whenever I spoke. As yet I have found no such insulator for sound as gutta percha is for electricity. Caoutchouc seems to be the best; but I have never been able by the use of any amount at my disposal to prevent the microphone reporting all it heard.

The question of insulation has now become one of necessity, as the microphone has opened to us a world of sounds, of the existence of which we were unaware. If we can insulate the instrument so as to direct its powers on any single object, as at present I am able to do on a moving fly, it will be possible to investigate that object undisturbed by the pandemonium of sounds which at present the microphone reveals where we thought complete silence prevailed.

I have recently made the following curious observation:—A microphone on a resonant board is placed in a battery-circuit together with two telephones. When one of these is placed on the resonant board, a continuous sound will emanate from the other. The sound is started by the vibration which is imparted to the board when the telephone is placed on it; this impulse, passing through the microphone, sets both telephone discs in motion; and the instrument on the board, reacting through the microphone, causes a continuous sound to be produced, which is permanent so long as the independent current of electricity is maintained through the microphone. It follows that the question of providing a *relay* for the human voice in telephony is thus solved.

The transmission of sound through the microphone is perfectly duplex; for if two correspondents use microphones as transmitters and telephones as receivers, each can hear the other, but his own speech is inaudible; and if each sing a different note, no chord is heard. The experiments on the deaf have proved that they can be made to hear the tick of a watch, but not, as yet, human speech distinctly; and my results in this direction point to the conclusion that we only hear ourselves speak through the bones and not through the ears.

However simple the microphone may appear at first glance, it has taken me many months of unremitting labour and study to bring to its present state through the numerous forms, each suitable for a special object. The field of usefulness for it widens every day. Sir Henry Thompson has succeeded in applying it to surgical operations of great delicacy; and by its means splinters, bullets, in fact all foreign matter, can be at once detected. Dr. Richardson and myself have been experimenting in lung and heart diseases; and although the application by Sir H. Thompson is more successful, I do not doubt that we shall ultimately succeed. There is also hope that deafness may be relieved. For telephony articulation has become perfect, and the loudness increased. Duplex and multiplex telegraphy will profit by its use; and there is hardly a science where acoustics has any direct or indirect relation which will not be benefited. And I feel happy in being able to present this paper on the results obtained by a purely physical action to such an appropriate and appreciative body as the Physical Society.

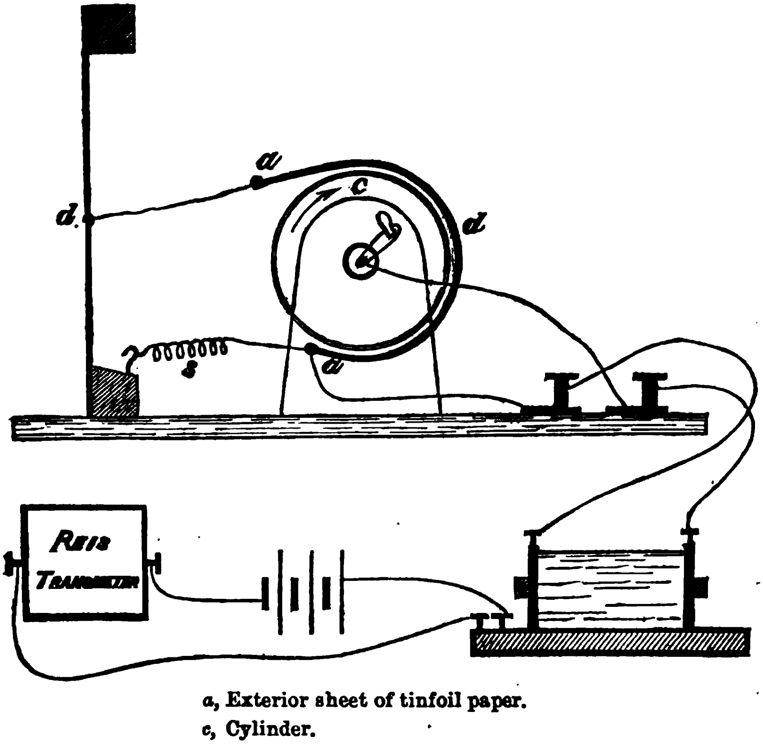
In conclusion, allow me to state that throughout the whole of my investigations I have used Prof. Bell's wonderfully sensitive telephone instrument as a receiver, and that it is owing to the discovery of so admirable an appliance that I have been enabled to commence and follow up my researches.

The SECRETARY then read the following paper:—

A STATIC INDUCTION TELEPHONE.

By W. MOON, Associate.

While experimenting with Dr. Wright's tinfoil paper telephone, I noticed that, when the two sheets of paper were charged with high tension electricity, they were attracted and clung to each other, and that it required considerable force to pull them apart. This fact suggested to me the construction of the telephone I have the honour of bringing before the Society to-night.



a, Exterior sheet of tinfoil paper.
c, Cylinder.
d, Diaphragm.
s, Spring.

One sheet of tinfoil paper is fixed to a cylinder, with the metal surface inside, and another sheet of similar paper rests on the surface of the thus covered cylinder, with its metal surface outwards, and is attached by a thin cord to the centre of a diaphragm, so that, on rotating the cylinder, the diaphragm is pulled in the direction of rotation of the cylinder by the friction between the two surfaces of paper.

By charging the two sheets of paper with high tension electricity from an induction coil, the friction is increased, and the diaphragm drawn in at each vibration of a rheotome, so that the vibrations of the rheotome are exactly reproduced in the telephone; and, by substituting a Reis transmitter in the primary circuit in place of the rheotome, the pitch of each musical note of the transmitter is exactly reproduced in the telephone.

The loudness of the telephone is proportional to the surfaces of the paper in contact, "or to the capacity in farads," and most probably to the square of the tension of the charge. It is, however, necessary that the power of the induction coil used should be proportional to the capacity of the telephone, as, if the coil is too powerful, sparking takes place between the two surfaces of the paper. With the larger telephone on the table, I find that a coil of ¼ in. spark gives the best result.

Mr. Kempe has improved the loudness of the telephone by pressing the two surfaces together by a spring, and also by using powdered chalk between the two surfaces of the paper.

I have tried many different forms of transmitters that Mr. Preece has kindly supplied me with, and find that the original form of Reis transmitter gives the best result.

A unanimous vote of thanks was passed to Mr. Moon for his paper.

The PRESIDENT: The meeting will now adjourn to 10th May, 1883, when a paper will be read on "Electric Motors, and their Government," by Professors W. E. Ayrton and J. Perry.

The Members proceeded to examine the various apparatus laid on the table by Mr. J. Munro, Mr. Moon, and Prof. A. Jamieson.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY DURING APRIL, 1883.

BY ALFRED J. FROST, *Librarian.*

(*Works marked with an Asterisk* (*) *have been purchased. They comprise, with two or three exceptions, a collection of Pamphlets made by the late* C. V. WALKER, *F.R.S., Past President.*)

* **Anon.** The First Steps in Electrotype. 32mo. 34 pp. *London.*

* ——— [J. G. West (?)] A Brief Description of Electrography. Part II. 18mo. 36 pp. Plate. *London.*

* ——— [Smith, Archibald] (?) Swinging Ship for Deviation. 8vo. 16 pp.

——— [F.R.S.] Most Important Errors in Chemistry, Electricity, and Magnetism pointed out and refuted; and the Phenomena of Electricity and the Polarity of the Magnetic Needle accounted for and explained. 8vo. 47 pp. *London*, 1846

——— [Memb. Inst. Civ. Eng.] The Atlantic and South Atlantic Telegraphs. 8vo. 32 pp. *London*, 1859

——— Télégraphie Domestique. Instruction sur la pose et l'entretien des sonnettes électriques. 8vo. 61 pp. *Paris*, 1865

* **Airy** [G. B.] Lecture on the Determination of the Longitude of the Observatory of Cambridge by means of galvanic signals. 8vo. 4 pp. [Extract from *Monthly Notices of the Royal Astronomical Society*, June, 1853.] *London*, 1853

* **Alexander** [Wm.] Plan and Description of the original Electro-magnetic Telegraph; with prefatory note to the Royal Commissioners of the Exhibition of the Works of Industry of all Nations, and relative documents. 8vo. 30 pp. *London*, 1851

Ampère. [*Vide* Hare, R.]

Atkinson [E.] [*See* Ganot.]

* **Atlantic Telegraph.** Letter from a Shareholder, signed F.M., to Mr. Whitehouse, and his reply. 8vo. 11 pp. *London*, 1858

——— [*Vide* Hunt, Robert.]

——— [*Vide* Anon., Memb. Inst. Civ. Eng.]

Atlantic Telegraph Company. [*See* Whitehouse, E. O. W.]

* **Baddeley** [Wm.] **and Walker** [C. V.] Correspondence in *Mechanics' Magazine*, August to November, on "Lightning Conductors on Royal Exchange." *London*, 1844

* **Bain** [Alex.] A Short History of the Electric Clocks, with Explanations of their Principles and Mechanism, and Instructions for their Management and Regulation. 8vo. 31 pp. *London*, 1852

* **Beardmore** [S.] The Globe Telegraph. An Essay on the use of the Earth for the Transmission of Electric Signals. 8vo. 47 pp. Plates. *London*, 1859

Becquerel. [*Vide* Jacobi.]

Berzelius. [*Vide* Hare, R.]

* **Board of Trade.** Circular dated October 15, 1856. On Deviation. 8vo. 7 pp. Plates. *London*, 1856

* **Bolton** [Capt. F.] Telegraph Code. 8vo. 8 pp. *London*, 1866

——— Army Industrial Exhibitions. 8vo. 16 pp. [*Journal Royal United Service Institution.* Vol. VIII.] *London.*

——— On Telegraphy for Naval and Military Purposes. 8vo. 25 pp. [*Journal Royal United Service Institution.* Vol. VII.] *London.*

Boswall. [*Vide* Glass and Boswall.]

* **Boulay.** Notice sur une nouvelle pile électrique à courant constant. 8vo. 15 pp. *Paris.*

Bonelli [G.] [*Vide* Jundzill.]

Breguet fils [L.] **et Séré** [V. de]. Télégraphique électrique, son avenir poste aux lettres électriques, journaux électriques, suivi d'un aperçu théorique de télégraphie. 8vo. 75 pp. *Paris*, 1849

* **Brett** [John Watkins]. On the Submarine Telegraph. 8vo. 11 pp. [Royal Institution, March 20, 1857.] *London*, 1857

Bright [Sir C. T.], M.P. The Telegraph to India, and its Extension to Australia and China. With an Abstract of the Discussion upon the Paper. 8vo. 67 pp. Plate. [*Proc. Inst. Civ. Eng.* Vol. XXV. Session 1865-66.] *London*, 1866

British Association. Provisional Report of the Committee on Standards of Electrical Resistance. 8vo. 11 pp. [British Association Report, 1862.] *London*, 1862

* **Brooke** [C.] On the Dynamical Theory of Electricity. 8vo. 4 pp. [*Phil. Mag.*, Nov. and Dec., 1866.] *London*, 1866

——— Remarks on the Nature of Energy, and the Correlation and Transmutations of its various Physical Forms. Being the introductory chapter to a new edition of "The Elements of Natural Philosophy." 12mo. 14 pp. *London*, 1869

* **Capello** [Senhor] **and Stewart** [Balfour, M.A., F.R.S.] Results of a Comparison of certain Traces produced simultaneously by the Self-recording Magnetographs at Kew and at Lisbon, especially of those which record the magnetic disturbance of July 15, 1863. 8vo. 12 pp. [*Proc. Roy. Soc.* No. 60, p. 111.] *London*, 1863 (?)

* **Caplin** [Dr.] Electro-chemical Bath and Medico-electrical Establishment. 18mo. 24 pp. *London.*

* **Chalmers** [Charles]. Notes of Experiments, with Thoughts on Electricity. 8vo. 29 pp. *Edinburgh and London*, 1850

* **Chambers's Papers for the People.** Electric Communications. 8vo. 32 pp.

Chase [Pliny Earle]. Numerical Relations of Gravity and Magnetism. 8vo. 6 pp.

* ——— Experiments upon the Mechanical Polarisation of Magnetic Needles under the Influence of Fluid Currents or "Lines of Force." 8vo. 16 pp. [*Proc. American Phil. Soc.*, July 21, Oct. 6, and Nov. 3.] 1865

• **Chevalier** [Charles]. Nouvelles instructions sur l'usage du Daguerréotype. Description d'un nouveau photographe et d'un appareil très simple destiné à la reproduction des épreuves au moyen de la galvanoplastie. 8vo. 78 pp. Plates. *Paris*, 1841

• **Clark** [Edwin]. Letter to Captain Huish as to Proposed Improvements in the Electric Telegraph System for the service of the London and North Western Railway Company. 8vo. 32 pp. *London*, 1854

—— [*Vide* Electric Telegraph Company.]

• **Clark** [Latimer]. Description of a System of Pneumatic Communication between Railway Passengers, Guards, and Engine Drivers. 12mo. 13 pp. *London*, 1865

• **Companion to the Almanac 1843.** Notices of the Electro-magnetic Telegraph. 8vo. 20 pp. *London*, 1842

—— **1853.** Electric Telegraphs. 12mo. 19 pp. *London*, 1853

Cooke [W. F.] (?) Electric Telegraph. Regulations to be strictly observed by the Workmen, etc., engaged on a Railway. 12mo. 12 pp

• **Deane** [John C.] Narrative of the Atlantic Telegraph Expedition, 1865. 17 pp. With 2 charts lithographed on board the s.s. "Great Eastern." [From] *London*, 1866 (?)

De Lauture. [*Vide* Lauture.]

• **Dent** [Edward J.] A Paper on the Patent Azimuth and Steering Compass 8vo. 7 pp. [Presented to the meeting of the British Association at York. Read September 30, 1844.] *London*, 1844

• **Dering** [G. E.] Magnetism. A sketch of the history and principles of the science and its various useful applications, including those of the Mariner's Compass and Electric Telegraph. 12mo. 35 pp. [Lecture delivered at the Hatfield Mutual Improvement Society, January 31, 1853.] *Hertford*, 1853

Dove [Prof.] [*Vide* Hare, R.]

Dufay. [*Vide* Hare, R.]

• **Edinburgh Review** [October, 1844]. On the Nature of Thunderstorms, and on the Means of Protecting Buildings and Shipping against the destructive effects of Lightning. By W. S. Harris. 8vo. 30 pp. *Edinburgh* (?), 1844

• —— [October.] The Electric Telegraph. 8vo. 32 pp. [Vol. XC. No. CLXXII., pp. 434-472] *Edinburgh*, 1849

• **Electric Telegraph Company.** New System of Train Signals. 8vo. 10 pp. [By Edwin Clark, Engineer.] *London*, 1854

• **Elice** [F.] Notizie Elettriche. 8vo. 12 pp. *Genova*, 1844

• —— Osservazioni ed esperienze sull' elettricita. 8vo. 16 pp. *Genova*, 1844

• —— Nuovo metodo per eccitare l'elettricità collo schioppo e proposta di un fulmine artificiale. 8vo. 8 pp. *Genova*, 1843.

• —— Osservazioni sui Parafulmini. 8vo. 11 pp. *Genova*, 1843

Elkington & Co. Gilding and Silvering certain Metals. 8vo. [*London Journal and Rep. of Arts, Sciences, and Manufactures.*] *London*, 1841

• —— On the Application of Electro-Metallurgy to the Arts. 12mo. 32 pp. *London*, 1844

* **Ellis** [Wm.] Account of some Experiments showing the change of rate produced in a clock by a particular case of magnetic action. 8vo. 7 pp. [*Philosophical Magazine*, May, 1863.] *London*, 1863

* —— Lecture on the Greenwich System of Time Signals. 8vo. 22 pp. [*Horological Journal.*] [Delivered at Society of Arts, Feb. 24, before members of the British Horological Institute.] *London*, 1865

Fahie [J. J.] An Episode in the Early History of the Telegraph. 8vo. 8 pp. Historic Notes on the Telephone. 8vo. 8 pp. [From the *Electrician.*] *London*, 1883

* **Faraday** [M.] On the Physical Character of the Lines of Magnetic Force. 8vo. 28 pp. Plate. [*Phil. Mag.*, June, 1852, p. 401.] *London*, 1852

* —— Observations on Magnetic Force. 8vo. 10 pp. [Roy. Instn., Jan 21, 1853.] *London*, 1853.

* —— On Magnetic Hypotheses. 8vo. 3 pp. [Roy. Instn., June 9.] *London*, 1854

* —— On some Points of Magnetic Philosophy. 8vo. 7 pp. [Roy. Instn., Jan. 19.] *London*, 1855

* —— On some Points of Magnetic Philosophy. 8vo. 33 pp. [*Phil. Mag.*, Feb. 1855.] *London*, 1855

* —— On the Conservation of Force. 8vo. 14 pp. [Roy. Instn., Feb. 27, 1857.] *London*, 1857

* —— Further Observations on Associated Cases, in Electric Induction, of Current and Static Effects. 8vo. 5 pp. [*Phil. Mag.*, March, 1855.] *London*, 1855

* —— On Electric Conduction. 8vo. 9 pp. [Royal Instn., May 25, 1855.] *London*, 1855

* —— On Electric Induction. Associated Cases of Current and Static Effects. 8vo. 11 pp. [Royal Instn., January 20, 1854.] *London*, 1854

* —— [*Vide* Reade, J.]

* —— [Prof.] **and Riess** [Dr. P.] On the Action of Non-conducting Bodies in Electric Induction. 8vo. 17 pp. [*Phil. Mag.*, Jan.] *London*, 1856

Franklin. [*Vide* Hare, R.]

* **Galletti** [B.] **et Jounin** [A.] De l'électricité en general et de ses applications en particulier. 8vo. 31 pp. *Paris*, 1848

Galton [Francis]. The Telotype: a Printing Electric Telegraph. 8vo. 32 pp. *London*, 1850

* **Ganot.** Elementary Treatise on Physics Experimental and Applied. 10th Edition. 8vo. 972 pp. Plates. [Translated and edited by M. E. Atkinson.] *London*, 1881

* **Gassiott** [J. P.] Description of a Ruhmkorff's Induction Apparatus. 8vo. 8 pp. [*Phil Mag.*, June.] *London*, 1858

* —— On the Interruption of the Voltaic Discharge in vacuo by Magnetic Force. 8vo. 7 pp. [*Proc. Roy. Soc.* Vol. X., p. 269.] *London.*

* —— On the Luminous Discharge of Voltaic Batteries when Examined in Carbonic Acid vacua. 8vo. 12 pp. [*Proc. Roy. Soc.*, March, 1860.] *London*, 1860

* —— Experimental Investigations on the Stratified Appearance in Electrical Discharges. Effect obtained by varying the Resistance. 8vo. 12 pp. Plates. [*Proc. Roy. Soc.*, Dec. 11, 1862.] *London*, 1862

* **Gauss** [C. F.] **and Weber** [W.] Results of the Observations made by the Magnetic Association in the year 1836. Göttingen. 8vo. 18 pp. [*Taylor's Scientific Memoirs.* Vol. II. Part I.] *London.*

* **Gisborne** [F. H.] On the Value of Electricity for Conveying Orders from the Captain of a Steamer to the Steersman and Engineer. 8vo. 4 pp. Plate.

* **Glaisher** [James]. Philosophical Instruments and Processes, as represented in the Great Exhibition. 8vo. 40 pp. *London,* 1852

Glass and Another v. Boswell. Affidavits in Application for New Trial. 8vo. xxvi. + 204 pp. *London,* 1861

* **Glenn** [James.] The Real Nature of the Electric Fluid; Explained and Illustrated by Numerous Facts, and a Cause assigned for the Polarity of the Magnet. 8vo. 32 pp. New edition. *New York,* 1844

Glover [W. T.] Table showing the Relative Dimensions, Lengths, Resistances, and Weights of pure Copper Wire. 1 Sheet. *Manchester,* 1883

* **Gros, Le Baron** [J. B. Louis]. Lettre sur la Télégraphie électrique. Sm. 4to. 31 pp. Plates. *Paris,* 1856

* **Grove** [W. R.] On the Correlation of Physical Forces. 8vo. 52 pp. *London,* 1846

* **Halse** [W. H.] Medical Galvanism. 12mo. 12 pp. *London.*

* **Hare** [Rob.] Objections to the Theories severally of Franklin, Dufay, and Ampère, with an effort to explain electrical phenomena by statical, or undulatory polarisation. 8vo. 24 pp. An improved edition. Feb. 17, 1848. *Philadelphia* (?), 1848

* ——— A Letter to Professor Faraday on certain Theoretical Opinions. 8vo. 11 pp. [From the *American Journal of Science and Arts.* No. 1. Vol. XXXVIII.] *Philadelphia.*

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RULES AND REGULATIONS

FOR THE PREVENTION OF FIRE RISKS ARISING FROM ELECTRIC LIGHTING,

Recommended by the Council in accordance with the Report of the Committee appointed by them on May 11, 1882, to consider the subject.

MEMBERS OF THE COMMITTEE.

Professor W. G. Adams, F.R.S., *Vice-President.*
Sir Charles T. Bright.
T. Russell Crampton.
R. E. Crompton.
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Lieut.-Colonel C. E. Webber, R.E., *Past President.*

These rules and regulations are drawn up for the reduction to a minimum, in the case of electric lighting, of those risks of fire which are inherent in every system of artificial illumination, and also for the guidance and instruction of those who have, or who contemplate having, electric lighting apparatus installed in their premises.

The difficulties that beset the electrical engineer are chiefly internal and invisible, and they can only be effectually guarded against by "testing," or probing with electric currents. They depend chiefly on leakage, undue resistance in the conductor, and bad joints, which lead to waste of energy and the dangerous production of heat. These defects can only be detected by measuring, by means of special apparatus, the currents that are either ordinarily or for the purpose of testing, passed through the circuit. Should wires become perceptibly warmed by the ordinary current, it is an indication that they are too small for the work they have to do, and that they should be replaced by larger wires. Bare or exposed conductors should always be within visual inspection, and as far out of reach as possible, since the accidental falling on to, or the thoughtless placing of other conducting bodies upon such conductors, would lead to "short-circuiting," andthe consequent sudden generation of heat due to an increased current in conductors not adapted to carry it with safety.

The necessity cannot be too strongly urged for guarding against the presence of moisture and the use of "earth" as part of the circuit. Moisture leads to loss of current and to the destruction of the conductor by electrolytic corrosion, and the injudicious use of "earth" as a part of the circuit tends to magnify every other source of difficulty and danger.

The chief dangers of every new application of electricity arise from ignorance and inexperience on the part of those who supply and fit up the requisite plant.

The greatest element of safety is therefore the employment of skilled and experienced electricians to supervise the work.

I. The Dynamo Machine.

1. The dynamo machine should be fixed in a dry place.
2. It should not be exposed to dust or flyings.
3. It should be kept perfectly clean and its bearings well oiled.
4. The insulation of its coils and conductors should be practically perfect.
5. All conductors in the Dynamo Room should be firmly supported, well insulated, conveniently arranged for inspection, and marked or numbered.

II. The Wires.

6. Every switch or commutator used for turning the current on or off should be constructed so that when it is moved and left it cannot permit of a permanent arc or of heating.

7. Every part of the circuit should be so determined, that the gauge of wire to be used is properly proportioned to the currents

it will have to carry, and all junctions with a smaller conductor should be fitted with a suitable safety fuse or protector, so that no portion of the conductor should ever be allowed to attain a temperature exceeding 150° F.

8. Under ordinary circumstances complete metallic circuits should be used; the employment of gas or water pipes as conductors for the purpose of completing the circuit, should not in any case be allowed.

9. Bare wires passing over the tops of houses should never be less than seven feet clear of any part of the roof, and all wires crossing thoroughfares should invariably be high enough to allow fire escapes to pass under them.

10. It is most essential that joints should be electrically and mechanically perfect and united by solder.

11. The position of wires when underground should be clearly indicated, and they should be laid down so as to be easily inspected and repaired.

12. All wires used for indoor purposes should be efficiently insulated, either by being covered throughout with some insulating medium, or, if bare, by resting on insulated supports.

13. When these wires pass through roofs, floors, walls, or partitions, or where they cross or are liable to touch metallic masses, like iron girders or pipes, they should be thoroughly protected by suitable additional covering; and where they are liable to abrasion from any cause, or to the depredations of rats or mice, they should be efficiently encased in some hard material.

14. Where indoor wires are put out of sight, as beneath flooring, they should be thoroughly protected from mechanical injury, and their position should be indicated.

N.B.—The value of frequently testing the apparatus and circuits cannot be too strongly urged. The escape of electricity cannot be detected by the sense of smell, as can gas, but it can be detected by apparatus far more certain and delicate. Leakage not only means waste, but in the presence of moisture it means destruction of the conductor and its insulating covering, by electric action.

III. LAMPS.

15. Arc lamps should always be guarded by proper lanterns to prevent danger from falling incandescent pieces of carbon, and from ascending sparks. Their globes should be protected with wire netting.

16. The lanterns, and all parts which are to be handled, should be insulated from the circuit.

IV. DANGER TO PERSON.

17. Where bare wire out of doors rests on insulating supports, it should be coated with insulating material, such as india-rubber tape or tube, for at least two feet on each side of the support.

18. To secure persons from danger inside buildings, it is essential so to arrange and protect the conductors and fittings, that no one can be exposed to the shocks of alternating currents of a mean electro-motive force exceeding 100 volts, or to continuous currents of 200 volts.

19. If the difference of potential within any house exceeds 200 volts, the house should be provided with a "switch," so arranged that the supply of electricity can be at once cut off.

By Order of the Council.

F. H. WEBB, *Secretary.*

Offices of the Society,
4, The Sanctuary, Westminster.
April 11*th*, 1883.

ORIGINAL COMMUNICATIONS.

OBSERVATIONS OF EARTH CURRENTS.

By E. O. Walker, Member.

I have compiled the following notes on the best conditions under which to observe earth currents, which may be of use if the Council decide to issue instructions to observers:—

1. Galvanometer to be unsensitive to inductive disturbances, but sensitive enough to give readable deflections with an electromotive force of 1 volt through 5,000 ohms external resistance.
2. Galvanometer to be of low resistance.
3. Earth plates to be of the same metal at both of observing stations, and of the lowest possible resistance.
4. Line selected for observation to be approximately east and west; geographical position to be plotted on chart.
5. Line to be of thick wire, of lowest resistance possible; or two or more wires coupled together to reduce resistance of circuit.
6. The longer the line, the larger the gauge to be selected. Observations are easily made on a line 1,000 miles in length of No. 1 B.W.G., but not so on the same length of No. 5½ or 8 B.W.G. For small gauges a length of 200 miles is ample. On the other hand, very short lines to be avoided, since polarisation and galvanic action are more declared on them.
7. That line to be selected which is farthest removed from other wires, to avoid extraneous disturbances.
8. Difference of elevation above sea level of the two terminal stations to be noted, and contour of the track of country crossed by the line. The greater the difference of elevation, the greater the difference of potential.
9. Intermediate offices, if possible, to be joined direct, but in any case the resistance in circuit to be noted, and preserved constant for all observations made.

Belgaum, *20th March*, 1883.

TRANSLATION OF LETTER FROM MONSR. J. JOUBERT,

Professor of Physics at the College Rollin, Paris,

Addressed to a Member of the Fire Risks Committee in reply to inquiries in connection with Rule 18 *of the Committee's Rules.*

Paris, *February* 27*th*, 1883.

Dear Sir,

I must ask you to excuse my not having sooner answered your letter. From a wish to make my reply as complete as possible, I had asked for some information, which has not yet reached me; as soon as it does I will forward it, if worth doing so.

The question you raise is very difficult of solution in the absence of sufficient experimental data. Although the number of accidents have unfortunately been considerable, yet they have not given rise to any inquiry sufficiently carefully conducted as to point out how the accidents are to be avoided. What may be said on this subject rests, therefore, more on assumptions than on established facts.

I will, however, begin my answer by explaining my own views on the subject.

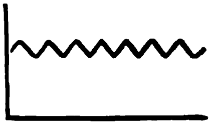
In my opinion electric machines, as far as regards public safety, ought not to be divided into *continuous* and *alternating* current machines, but into *constant current* machines and *variable current* machines; a battery being a type of the former class.

I believe that, without grave danger, and for *a very short time*, the two poles of a powerful battery may be placed in communication. The late Professor Despretz, of the Sorbonne (who made numerous experiments with 600 Bunsen cells in series, *i.e.*, with an E.M.F. of 1,000 volts), stated that on more than one occasion he and his assistants, by accidentally closing the circuit through their own bodies, had received terrific discharges, and without the slightest ill effect.

M. Dubosq, the well-known electric lamp maker in Paris, I have heard speak similarly; he being in the habit of handling at the Grand Opera a battery of 800 Bunsen cells, coupled together in all sorts of ways. His reply to my present inquiry has not yet come to hand.

With greater reason, therefore, do I consider that there would be no danger in touching one only of the poles of a still more powerful battery.

The same would evidently hold for a constant current machine. I do not, however, know of any practical one. All dynamo machines give off currents which are more or less variable. In "Gramme" and in "Siemens" machines the intensity of the current may be represented as in the sketch, while in the "Brush" machine the variations are much greater.



I firmly believe that the physiological action, and consequently the danger, arises principally, if not entirely, from the extreme rapidity with which the variations of the E.M.F. follow each other; and that this action is greater in proportion as these variations are more numerous and of greater amplitude. From this point of view *it is quite immaterial* whether the successive currents are always in the same direction or whether they are alternately in contrary direction.

A "Brush" machine appears to me to be quite as dangerous as an alternating current one, even though the former is classed among continuous current machines.

To be brief, I believe that the danger which may arise from a machine depends only in a slight degree on the absolute difference of potential between its two poles; but that it depends principally on the amplitude and frequency of the variations having this difference.

Having thus explained my opinion generally, I will attempt to reply to the different questions asked me.

1. "*Do you approve, as a maximum limit, of* 200 *volts for continuous currents, and of* 60 *volts for alternate currents? May the latter limit be pushed to* 130 *volts?*"

If by a "continuous" current is meant what I have termed a "constant" current, the first-named limit may be considerably exceeded. As to "variable" currents, I will return to them as soon as I have replied to the following questions.

2. "*What do you understand by a limit of* 60 *volts? What meaning, think you, would be attached to such an expression?*"

Personally I should consider a limit of 60 volts to mean a maximum difference of potential equal to 60 volts. Thus, in a Gramme or a Siemens alternate current machine, where the difference of potential between its two poles varies sensibly as sin. $2\,\pi\,\frac{t}{T}$ (T being the entire period), I should understand that the maximum ordinate representing this difference ought not to exceed 60 volts.

I would, however, remark that a machine under such a condition *would not be capable of supporting an arc*, as this maximum difference is very nearly that which must *necessarily* exist between the two carbons (with alternate currents, of course).

As to the interpretation which might be given to the expression *a maximum limit of* 60 *volts*, I think that it would be understood, not that the maximum ordinate should not exceed 60 volts, but only that the *mean ordinate* should not do so.

But in the question actually at issue it is, I believe, the maximum, and not the mean ordinate, which ought to be taken into consideration.

3. "*While holding one conductor only, would one be exposed to a difference of potential of* 60, *or of twice* 60 *volts?*"

If one was in perfect communication with the earth, I believe the former case would arise, as one would then have but one conductor affording a difference of potential of 60 volts, and it would be indifferent whether the successive variations, starting from zero, were positive or negative in direction.

If, however, one was insulated, the second case would arise, since the body would then pass alternately from a potential of + 60 to a potential of — 60; and it would be subjected to variations of potential of 120 volts. I am, however, quite convinced that in neither case would there be the slightest danger in being exposed to much greater variations of potential. I can quote my own experience on this point. When carrying out the experiments described in my pamphlet, I worked with an alternating current Siemens machine which was *perfectly insulated;* one of the poles being permanently connected to the frame of a portable electrometer of Sir Wm. Thomson, the other with the electrode of the instrument. By turning the micrometer I was placed in communication with the poles of the machine. When the electrometer was in communication with the earth I felt nothing at all, and only a slight tingling when it was insulated. In these experiments, however, the difference of potential was carried nearly up to 1,200 volts, alternately + 1,200 and − 1,200.

I took good care to make sure of the perfect insulation of the second pole; otherwise the result might have been a very severe shock.

In illustration of this I will cite the following fact. An alternate current Gramme machine was working so that the maximum ordinate representing the difference of potential of the two poles of the machine, when open, reached about 500 volts. A young man, wishing to cut off the current from the commutator, which was close to the machine, and not having at hand the key with an insulated handle which was generally used, took a piece of a file without any handle; he was struck to the ground. There was leakage going on from the other pole to earth.

4. "*In your pamphlet you make use of the expression 'mean electro-motive force.' In what sense do you mean this? and, in particular, what do you understand by a mean electro-motive force of* 60 *volts?*"

I have termed "mean electro-motive force" the mean ordinate, irrespective of its sign; an ordinate which, if the electro-motive force is represented by a sinoidal curve, is equal to a fraction, $\frac{2}{\pi}$, of the maximum ordinate. In the case of a mean electro-motive force of 60 volts, it would therefore be equal to about 90 volts.

The above are some of the suggestions which your letter has given rise to. I shall be happy to afford you any further explanations. Moreover, if anything additional, of interest, comes to hand, I will at once send it to you.

Yours truly,

J. JOUBERT.

MANSION HOUSE BUILDINGS,
LONDON, E.C., *April 10th*, 1883.

The Secretary,
Society of Telegraph Engineers
and of Electricians.

DEAR SIR,

In reply to your question as to my views on the "effects of the quantity and magnetic charge" of a current when considering its effects on human life,

I think there are two distinct modes in which the human body, or other vital organism, is affected by the passage through it of an electric current.

First. When it forms part of a derived circuit to the main conductor in such manner that, when the latter is severed, the whole extra current (of any current normally passing through the conductor) takes effect upon it (the organism).

In these cases the organism receives a blow, or shock, as it is usually called, and this shock may be severe even when the E.M.F. of the normal current is comparatively low, as I think that there is no doubt that it is mainly due to the extra current at the moment of interruption.

Undoubtedly, this extra current bears a definite ratio to the normal current, and probably is directly proportionate to the magnetic moment of the whole circuit.

This may be small, if the wire forming the conductor be short; but if it be coiled on itself, or if any part of it forms a coil into which an iron core is introduced, the magnetic moment of the whole system being increased, the extra current is also increased, and it is to this that I referred when I called attention to the necessity of considering the effects of "magnetic charge."

In most of the arc lighting systems using the direct current, the magnet coils of the dynamos are in series circuit with the main circuit, therefore any person accidentally receiving the shock through any portion of his body receives one in degree proportionate to the extra current due to the magnetic moment of the entire system.

I myself have frequenly taken the shock from a Burgin dynamo machine working five arc lamps in series; the normal E.M.F. being 250 volts. I received the shock in the following manner :—When steadying the arc lamp with one hand, I raised the upper carbon with the other. I thus broke the circuit, and I received the extra current through my arms and chest. The shock was comparable to that from a large-sized Leyden jar. My men frequently receive shocks in this manner when adjusting arc lamps in which the dynamos give currents up to 25 ampères, with an E.M.F. of 300 volts; and although the shock is startling and unpleasant, yet I have never heard of the slightest injury to health resulting from it.

It will be readily seen that it is almost impossible for any one to receive this form of shock due to extra current when using incandescent lamps or arc lamps, if they are arranged in parallel circuit, as in this case. The interruption of any one branch leaves the other branch conductors intact, consequently the extra current on the interrupted branch is usually too small to be noticed; hence quantity and magnetic charge have no influence.

The *second* mode in which the human body is affected is when any part of it is so placed that it forms a conducting path, joining two conductors of different potentials (one of which may be the earth). In this case the continuous passage of the current through the organism gives rise to a sensation of tremor or burning tremor. This sensation varies greatly with different persons and with different parts of the body of the same person, and the severity of the sensation appears to depend on the

(1) E.M.F. or difference of potential between the two conductors;

(2) On the surfaces of contact between the organism and the conductor;

(3) On the intermittent or undulatory character of the current itself.

We made some experiments at our works with currents of different E.M.F. In these the conditions were as follows:—

The surfaces of contact were two 16 B.W.G. tinned copper wires spaced 3 inches apart over the edge of a board. These we bridged with the side of our hands. The current was supplied by a dynamo having a 60-part commutator, and was running at 1,600 revolutions. Therefore there were 1,600 commutations or undulatory movements of the current per second. We found that, even with moistened hands, the sensation from 100 volts was hardly noticeable. With 200 volts it was slight with dry hands, but too severe for the moistened hand when kept on for more than a few seconds. With 300 volts we could bear to bridge the wires with the dry hand for a second or two, but with a moistened hand the sensation was intolerable, but not so severe as to leave any after-effects. The sensation in this last case was as if the wires were heated and searing their way into the flesh.

The conclusions I draw are, that in considering risks from shocks due to extra currents, systems supplying an E.M.F. of 300 volts with a quantity of 25 ampères are certainly not dangerous; and that in considering the limit of E.M.F. in currents employed in incandescent or parallel arc lighting, the limit of safety probably lies between 300 and 400 volts.

Yours faithfully,

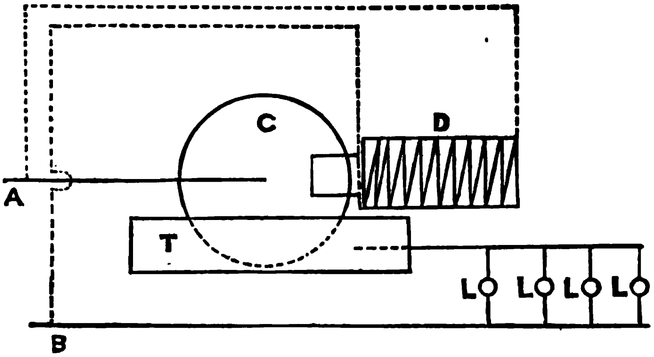
R. E. CROMPTON.

ABSTRACTS.

J. BAUMANN—ELECTRIC-ENERGY-METER.

(*Centralblatt für Elektrotechnik, B. V., No.* 1, *p.* 19.)

The apparatus consists of a Barlow's wheel, which rotates between the poles of a fixed electro-magnet. The wheel and mercury trough are in the main circuit, while the coils on the electro-magnet form a shunt to any portion of the circuit in which it is desired to measure the energy expended. The accompanying figure shows clearly the apparatus and the method of connection. C is the wheel, T the trough, D the electro-magnet, and L the lamps. The main circuit is shown by full lines, while the dotted lines show the shunt circuit taken off at the points A and B.



Suppose the current is i, and the intensity of the magnetic field is I, then the distance L, traversed by a point on the periphery of the wheel during the time t, is

$$L = \int_0^t a i I dt,$$

where a is a constant dependent on the construction of the individual apparatus.

If I varies directly as the difference of potential of the two points where the shunt is connected, we shall have

$$L = a\int_0^t i(v - v')dt,$$

The condition that I should vary as $(v - v')$ is attained by making the coils of the electro-magnet which form the shunt circuit of a very considerable resistance. If the axle of the Barlow's wheel is connected with a system of wheelwork, and the whole apparatus with a clock in such a way that the passage of the current starts it, while the cessation of the current stops it, the energy expended during the time the apparatus is in action can be recorded. In order to obtain regular working, it is necessary that the friction of the axle on its supports should be constant. This may be sufficiently nearly obtained by making the wheel as heavy as may be, and by arranging that the speed of the wheel does not vary too much for the extreme currents used with the apparatus.

F. GATTINGER.—RATIONAL METHOD FOR MEASURING THE RESISTANCE OF AN EARTH

(*Centralblatt, B. V., No. 1, p. 21.*)

The author is of opinion that the present method, by using only one comparative measurement, is insufficient, and may lead to error, as, for example, in measuring the earth of a lightning conductor; and he proposes an improved method.

Suppose the resistance of the earth, x, is to be determined, two other earths, y and z, are made, which may be of high resistance, so that iron water-pipes and such like can be used. As a rule, the resistance of the copper wire conductors may be neglected, but for very exact measures it should be taken into account.

First, the resistance of the circuit containing the two earths x and y is determined—suppose it to be A. Then find the resistances, B, C, of the two circuits x and z, y and z.

From the three equations, $x + y = A$, $x + z = B$, $y + z = C$, we have

$$x = \frac{A + B - C}{2};$$

or, in words, add the resistances of the two circuits x and y and x and z, and subtract from the sum the resistance of y and z, the remainder divided by 2 is the resistance of the earth, x.

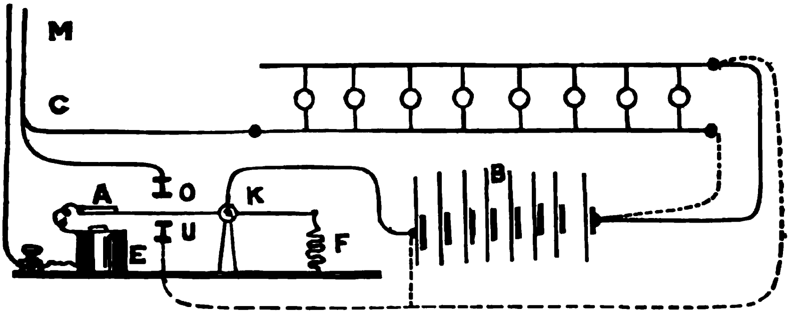
EMIL BOETTCHER.—AUTOMATIC COMMUTATOR FOR SECONDARY BATTERIES.

(*Centralblatt für Elektrotechnik, B. V., No. 3, p. 62.*)

This commutator allows of varied use, but is more especially meant to be employed in cases where the dynamo machine used for charging the cells runs at varying speeds—as when it is driven from the wheels of a train, for example. Where both a dynamo machine and a set of accumulators are connected parallel with a number of glow lamps, it can be used to automatically alter the connections, so that only the accumulators are in circuit with the lamps, the machine being cut out. Or it can be used when secondary batteries are being charged to cut off connection, and so prevent reversal of the poles of the dynamo by the return current from the secondary battery, should there be any irregularity in the speed of the former.

From the dynamo machine, M, the current passes into the coils of the electro-magnet E, then through a pliable strip of copper to the armature A, which is supported by a hinge on the insulated pillar K, and which, if the current is too weak or does not circulate, is drawn down at one end by the spiral spring F, and thus presses against the contact-screw O. The circuit is now closed, either through the screw K, if the battery and lamps are connected in series, or through the screw in the lower contact, U, if they are connected parallel. In either case a newly developed current is short-circuited through O and C. As soon as the current becomes sufficiently strong, it breaks this short-circuit, since the electro-magnet attracts its armature and interrupts the contact at O. The current then passes from the armature either through K and the battery and lamps, if these are connected in series, or through the lower contact, U, on which the attracted armature rests, if they are connected parallel, and so back to the machine. This second case is shown by the dotted lines in the figure. If, owing to a decrease in the strength of the current, the electro-magnet releases the armature, which again makes contact at O, the machine is again short-circuited. It is advisable to introduce between O and C a resistance nearly equal to that of the normal external circuit, in order to avoid too great alterations in the strength of the current.

The current from the battery, on the other hand, in the former case enters by K into the lever of the armature, and then passes through the upper contact, O, and the connecting wire, O C, through the lamps, and so back to the battery, as shown by the full lines. In the second case, shown by dotted lines, when one pole of the battery is joined to U, the current passes directly through the lamps without traversing the apparatus at all.



When the battery alone is being charged, the connections are made as shown by the dotted lines, the lamps of course being cut out.

In order to regulate a too powerful current, two apparatus are connected up behind each other. In the first one, which has its electro-magnet connected direct to the machine, a resistance is inserted between the upper contact, O, and the return wire; in the second apparatus, a resistance is inserted between the lower contact, U, and the lead to the battery and lamps. For a weak current the dynamo will be short-circuited through the upper contact and the resistance, and at the same time the circuit of the battery is completed through the lamps; while as the current increases the armature will be attracted by the electro-magnet, and the current will be shunted through the second apparatus. In this second apparatus, when the pull of the electro-magnet, due to increase of current, overcomes the opposing spring F, and the armature lever makes contact with U, the resistance is shunted in.

In conclusion, the author considers the proportion of current which should be furnished by the dynamo and by the battery.

E. DORN—REDUCTION OF THE SIEMENS UNIT TO ABSOLUTE MEASURE.

(*Annalen der Physik und Chemie, B. XVII., H. 5, No. 13, 1882, pp. 773-816.*)

The author gives at length an account of the experiments, extending over several months, which he has made to determine accurately the value in absolute measure of the Siemens mercury unit. Every possible precaution was taken in making the observations, the details of which must necessarily be omitted in an abstract.

The method employed was an amplification of Weber's second method (see Journal, No. 43, abstract p. 407) as used by Kohlrausch. The only measurements of the dimensions of coils which had to be made, were those of one layer of a large tangent galvanometer, so that the objections brought forward by Wiedemann on this score did not hold. The chief source of uncertainty in the author's method is, that the square of the horizontal component of the earth's magnetism enters into the calculation. However, while in Weber's plan it appears in the numerator, in the author's it appears in the denominator; so that by a combination of the two methods of observation made at the same place, the value of the horizontal component might be eliminated.

The general scheme of the method is as follows:—

Let q be the moment of rotation of unit current on the magnet of the galvanometer, k the moment of inertia of the magnet, T_0 its time of oscillation when the circuit is open, λ and λ_0 the logarithmic decrement for the absolute resistance, w, of the circuit, and with the galvanometer on open circuit, then

$$q^2 = \frac{2\,w\,k}{T_0}\left\{\lambda\sqrt{\frac{\pi^2+\lambda_0^2}{\pi^2+\lambda^2}}-\lambda_0\right\}$$

If the deflection for the constant current i is ϕ in absolute angular measure, and if we put $\phi = p\,i$, then for very small deflections p is a constant which depends upon q by the equation

$$q = p\,\frac{\pi^2 k}{T_0^2}$$

The principle of the method is the determination of p by comparison with a tangent galvanometer; and, viewed in this light, it may be looked upon as a modification of Weber's third method.

For this purpose the tangent galvanometer with n convolutions of mean radius R is connected up in the circuit of the main current I, which divides itself between a resistance w_n and the branch containing the mirror galvanometer, and having a total resistance w_g. Let H be the horizontal component of the earth's force, θ the deflection of the tangent galvanometer, ϕ that of the mirror galvanometer, then

$$I = \frac{H\,R}{2\,\pi\,n}\tan.\,\theta$$

$$i = \frac{w_n}{w_n + w_g}\,.\,I$$

Finally, if the resistance w in the first equation equals W Siemens units, and if one Siemens unit equals σ absolute units, by elimination we obtain the final equation:

$$\sigma = \frac{2\,\pi^6\,n^2}{H^2\,R^2} \cdot \frac{\phi^2}{\tan.\,\theta^2} \left(\frac{w_g + w_n}{w_n}\right)^2 \cdot \frac{K}{T_o^2\,W \left\{ \lambda \sqrt{\frac{\pi^2 + \lambda_o^2}{\pi^2 + \lambda^2}} - \lambda_o \right\}}$$

This equation shows how σ depends on the several quantities to be measured, and what measurements are necessary.

Before beginning the observations, H has to be determined for the place where the tangent galvanometer is set up, also the same thing has to be done for R and K. Of the several resistances, it is necessary to know the ratio w_n / w_g, and W in Siemens' units. The observations, properly so called, are the comparison of the mirror with the tangent galvanometer, the determination of the time of oscillation and of the logarithmic decrement for four different resistances. In all cases several series of such observations were made, and the mean values only were used in the calculations.

The corrections to be taken account of were the dependence of the sensibility and damping of the galvanometer needle on the amplitude, the influence of the extra current, the true position of the needle of the tangent galvanometer, etc.

In consequence of these corrections, the last equation given above was not used for the calculations, but the corrections were introduced into each step of the measurements, leading finally to the equation

$$\sigma = \frac{\pi^4\,(p_o\;H)^2\,K}{2\,T_o^2\,H^2\;\Omega}$$

where H refers to the horizontal component of the earth's force, p_o H to the galvanometer and earth's force jointly, K is the moment of the inertia of the magnet, T_o its time of oscillation, and Ω refers to the logarithmic decrement and damping.

The following table gives the values obtained:—

Date.	p_o H.	H.	Temp.	T_o	Ω	σ=10¹⁰ ×	Difference.
Jan. 9 ...	19·062	1·9363	11·36	12·4851	0·43712	0·94834	+ 0·00009
April 3 ...	19·047	1·9338	10·80	12·4903	0·43697	0·94852	+ 0·00027
„ 4 ...	19·050	1·9378	10·65	12·4780	0·43684	0·94798	− 0·00027
„ 5 ...	19·051	1·9376	10·84	12·4789	0·43681	0·94807	− 0·00018
„ 12 ...	19·055	1·9395	11·02	12·4744	0·43637	0·94865	+ 0·00040
„ 15 ...	19·055	1·9381	11·42	12·4799	0·43672	0·94797	− 0·00028
„ 16 ...	19·049	1·9380	11·75	12·4807	0·43622	0·94837	+ 0·00012
„ 18 ...	19·051	1·9367	11·80	12·4853	0·43655	0·94812	− 0·00013
					Mean	0·94825	+ 0·00022

The mean value of K at 12° temperature was found to be 1·70926 × 10^9, and the alteration for 1° = 0·0000574 × 10^9.

The observations were carried out in a large vaulted room in the University of Breslau, and, to avoid all disturbances, between 9 o'clock at night and 1 o'clock in the morning.

The time was measured by a very accurate clock with a compensation pendulum, which was standardised by the clock of the Breslau Observatory. All measurements of length were made with rods directly compared with the standards, and due allowance for effects of temperature were made. The scales were made of glass, and were each nearly one mètre long. The distances from the galvanometer mirrors to the scales were read by means of microscopes with crossed fibres. The observations were so arranged that a series of galvanometric measurements alternated with determinations of the earth's force; and for these latter the magnetometer was set up exactly in the position which would be occupied by the tangent galvanometer. A brass wire fastened to the ceiling carried the torsion circle, the mirror, and the boat in which was laid the magnet. The remaining parts of the magnetometer stood on a long solid table. The greatest care was taken in determining the earth's force to remove all masses of iron from the neighbourhood of the magnetometer. The principal magnet used was 258·6 mm. long, had a diameter of 20·6 mm., and weighed 681·852 grammes.

The tangent galvanometer was one of Meyerstein's, adapted for the purpose. On the strong copper ring of the instrument were wound four convolutions of a copper wire of 1 mm. diameter, carefully insulated with paraffined cotton to a diameter of 1·3 mm. The mirror galvanometer was of the pattern proposed by Kohlrausch, and had 276 convolutions of 3 mm. copper wire arranged in 12 layers.

F. NIEMOLLER—ON THE RELATION BETWEEN THE E.M.F. OF A REVERSIBLE BATTERY AND THE PRESSURE EXERTED ON THE LIQUIDS IN IT.

(*Annalen der Physik und Chemie*, *B. XVIII.*, *H.* 3, *No.* 3, 1883, *p.* 429.)

That there is some connection is evident, if we reflect that the liquid in a cell is the less compressible the more salt it holds in solution. Suppose that the passage of the current produces a salt in the cell, at the same time that a high pressure is exerted on the liquid, then on account of the newly formed salt the liquid will acquire a greater elasticity, and will expand and thus do work. This work is not done if the liquid is not subjected to a high pressure. On account, therefore, of the work of expansion, the E.M.F. must be less with a high pressure than under the normal atmospheric pressure, at least with single fluid cells.

Such a reversible element is the one constructed by Helmholtz, in which the anode consists of calomel on mercury, and the kathode of amalgamated zinc in a solution of chloride of zinc. If with such a cell the original E.M.F. under normal pressure is E_0, then the E.M.F. at any other pressure may be found from the equation,

$$E_1 - E_0 - \frac{P^2 q h (1 + h)}{2 S} \times \frac{d \mu}{d h},$$

in which P is the pressure, q the quantity of salt deposited per second, h is the ratio of the weight of the water in the cell to the weight of the salt, S is the specific gravity of the solution, and μ is a constant dependent on the degree of concentration.

It will be seen, therefore, that the E.M.F. at the higher pressure is less, and that the decrease is proportional to the square of the pressure.

As a second example, the author considers the case of a reversible cell with one fluid, the electrodes being of the same metal,—for example, copper plates in sulphate of copper,—but the solution at the anode less concentrated than at the kathode. Then, according to Helmholtz, a current passes through the liquid from the less concentrated to the more concentrated solution.

F. KOHLRAUSCH — ON A METHOD OF DETERMINING THE EFFECTIVE AREA OF A COIL FROM GALVANIC MEASUREMENTS.

(*Annalen der Physik und Chemie, B. XVIII., H. 4, No 4, 1883, p. 518.*)

The ordinary method of finding the area of a coil from its dimensions, the number of convolutions of wire, and the total length, can no doubt lead to sufficiently correct results; but whether the true effective area can thus be determined with great accuracy is open to doubt. It has been objected that the upper layers place themselves in the hollows of the lower ones, and that the upper layers compress the lower ones. Also, it is not possible to verify any changes which may take place in the coil in course of time without unwinding and rewinding, which naturally destroys the coil.

One of the objects of measuring the effective area of a coil is the calculation of the action at a distance of a current traversing the coil. It is obvious, therefore, that from the measured action at a distance of the coil its area can be determined.

The action of the coil on a magnet can be compared in known ways with the horizontal intensity of the earth's magnetism. A current can also be compared with the horizontal intensity by means of a tangent galvanometer in the way first described by Weber; so that the measurement of the area of a coil is reduced to the measurement of a tangent galvanometer, if the same current traverses both. There need be no difficulty in obtaining the measurements of the tangent galvanometer, since it may be constructed with only one convolution, or at most with only one layer of wire.

The following is the general description of the method:—

One and the same current flows through both the coil and the tangent galvanometer. The axis of the coil should be placed east and west, and with its centre at a distance, a, from the needle of the galvanometer. The centre of the coil may be, 1st, east or west of the needle (first position), or, 2nd, north or south of the needle (second position).

Let I be the strength of the current, ϕ the deflection of the needle, M its magnetic moment, R the radius of the galvanometer, A the effective area of the coil.

The force exerted on the needle of the galvanometer by the current is

$$I\,M\,\frac{2\,\pi}{R}\cos.\phi \ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

In the first position the action of the coil on the needle is

$$2\,I\,M\,\frac{A}{a^3}\cos.\phi \ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

in the second position the coefficient 2 is wanting.

The action of the earth's magnetism and of the suspension on the needle is

$$-\,C\,M\sin.\phi \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

The deflection ϕ is produced by the joint action of the current in the ring of the galvanometer and in the coil; if the current only traverses the former, we shall have a deflection ϕ^1, which is to be considered negative, in case it is in the opposite direction to ϕ.

We then have $$C\tan.\phi = \left(2\,\frac{A}{a^3} + \frac{2\,\pi}{R}\right) I$$

and $$C\tan.\phi^1 = \left(2\,\frac{A}{a^3} - \frac{2\,\pi}{R}\right) I,$$

whence $$A = \frac{a^3\,\pi}{R}\cdot\frac{\tan.\phi + \tan.\phi^1}{\tan.\phi - \tan.\phi^1} \ldots\ldots\ldots\ldots\ldots \quad (4)$$

In the second position the expression on the right side of (4) must be multiplied by 2. This formula holds good on the assumption that a is so large that the dimensions of the coil do not need to be taken into account.

The author considers the various corrections to be introduced if the above assumption is not made, and arrives at the more accurate equation,

$$A = \frac{a^3\,\pi}{R} + \frac{\tan.\phi + \tan.\phi^1}{\tan.\phi \div \tan.\phi^1} \times \frac{1 + \epsilon + \zeta}{1 - \delta}$$

The following equations determine the values of ϵ, ζ, and δ:—

To find δ:—let l be the length of the coil, r_o and r_1 its inner and outer radii, and put

$$\frac{r_1^5 - r_o^5}{r_1^3 - r_o^3} = P; \quad \frac{r_1^7 - r_o^7}{r^3 - r_o^3} = P^1; \text{ then}$$

In 1st position

$$\delta = \frac{1}{a^2}\left(\frac{1}{2}\,l^2 - \frac{9}{10}\,P\right) + \frac{1}{a^4}\left(\frac{3}{16}\,l^4 - \frac{9}{8}\,l^2\,P + \frac{45}{56}\,P^1\right).$$

In 2nd position

$$\delta = \frac{1}{a^2}\left(-\frac{3}{8}\,l^2 + \frac{27}{40}\,P\right) + \frac{1}{a^4}\left(\frac{15}{128}\,l^4 - \frac{45}{64}\,l^2\,P + \frac{225}{448}\,P^1\right).$$

To find ϵ:—let b be the breadth, h the thickness of the galvanometer-ring; then

$$\epsilon = \frac{1}{6}\,\frac{h^2}{R^2} - \frac{1}{8}\,\frac{b^2}{R^2} + \eta,$$

where η is a correction for the conducting wires.

To find ζ:—let λ be the length of the needle, and d its diameter; then

In 1st position

$$\zeta = \frac{1}{3}\,\frac{\lambda^2}{a^2} + 0{\cdot}13\,\frac{\lambda^2}{R^2}; \text{ or } \zeta = \frac{1}{3}\,\frac{d^2}{a^2} + 0{\cdot}12\,\frac{d^2}{R^2}.$$

In 2nd position

$$\zeta = -\frac{\lambda^2}{a^2} + 0{\cdot}13\,\frac{\lambda^2}{R^2}; \text{ or } \zeta = -\frac{2}{3}\,\frac{d^2}{a^2} + 0{\cdot}12\,\frac{d^2}{R^2}.$$

In conclusion, the author considers some practical details of the method, and gives the particulars of a determination of the area of the original earth inductor of Weber.

R. LENZ—EFFECT OF PRESSURE ON THE RESISTANCE OF MERCURY.

(*Beiblätter, B. VI., St.* 10, *p.* 802.)

A thermometer tube, 1·2 m. long, was widened out at either end into two cups, and was filled with perfectly pure mercury which had been thoroughly dried and freed from air. The current was conducted to the cups by means of platinum hooks. The tube was placed in a piezometer, in which a pressure of 60 atmospheres could be attained, and which was surrounded with ice. Two platinum wires connected the platinum hooks with the binding screws of the piezometer. The resistances were measured by the Wheatstone bridge method, with a sine galvanometer of Kohlrausch, and an electro-dynamometer.

The resistance of the copper wires could be neglected, that of the platinum wires varied from 0·48 ohm to 0·256 ohm, according to the diameter. The resistance of the mercury column was about 7·11 ohms. On increasing the pressure from 0 to 60 atmospheres, the resistance decreased by 0·02 per cent. per atmosphere. The decrease is very nearly proportional to the pressure.

If the tube is filled by means of Weinhold's distillation apparatus in vacuum, the resistance in the first place is 0·12 per cent. smaller than if it is filled by sucking in the mercury.

E. BAZZI—HEAT DEVELOPED BY A VARIABLE CURRENT.

BLASERNA—*Idem.*

(*Beiblätter, B. VI., St.* 12, *p.* 953.)

The former author has measured the sum of the total heat effects of the primary current of an induction coil during a certain time of its variable condition, and of the extra current in the same direction. The experiments were made by cutting out the battery and replacing it by an equal resistance by means of Felici's commutator. The results observed agree very well with the values obtained by calculating the strength of the current from Helmholtz's formula, and then the quantity of heat developed from Joule's formula, thus showing that Joule's law holds good also for the variable period of an induced current.

Blaserna adds the remark that oscillatory charges might considerably prejudice the simplicity of this phenomena if the strength of the current were measured as a function of the time at individual moments.

G. FOUSSEREAU—THE ELECTRICAL RESISTANCE OF GLASS AT LOW TEMPERATURES.

(*Comptes Rendus, T.* 95, *No.* 5, 1882, *p.* 216).

A very small test tube, about 1 to 2 cm. in diameter, and closed at one end, is placed within a second wider test tube. The inner tube, as well as the space between the two, is filled to a known height with concentrated sulphuric acid, into which dip two platinum wires, which are carefully kept from touching the surface of the tubes above the acid. The whole is surrounded by a glass vessel, the air in which is thoroughly dried by the acid before beginning the experiment.

The apparatus is placed in an oil bath, itself surrounded by a sand bath or by a refrigerating mixture, so as to allow of any desired temperature being maintained.

One of the platinum wires is in connection with a battery of 1 to 100 cells, and the other is connected to a condenser of known capacity, the two poles of which are joined to the two mercury cups of a Lippmann electrometer, also of known capacity. The time is then observed which is required to produce a displacement of the mercury column of the electrometer, corresponding to a determined difference of potential.

If E is the E.M.F. of the battery,

ρ^1 and ρ_2 the interior and exterior radii of the test tube,

h the height of the acid,

C the sum of the capacities of the electrometer and condenser,

e the difference of potential shown by the electrometer,

r the specific resistance of the glass per cubic centimètre,

and allowing that the quantity of electricity transmitted through the glass in the time θ has charged the condenser, then we have

$$r = \frac{2 \pi h \mathrm{E}}{\mathrm{C}\, e \log._e \frac{\rho_1}{\rho_2}} \times \theta$$

At the moment of completing the circuit, the test tube is charged like a condenser. The inner layers gradually absorb the electricity necessary to bring them to the state of strain corresponding to the fall of potential between the two surfaces. The observations are not commenced until this state is reached.

The experiments were made with three kinds of glass, and led to the conclusion that the resistance could be expressed by the general equation,

$$\log. x = a - b\, t + c\, t^2.$$

1. For ordinary soda-lime glass of a density 2·589, the following results were obtained, the figures representing millions of meghoms:—

Temperature.		Resistance.
+ 61·2	...	0·705
+ 20	...	91·0
− 17	...	7970·0

and the general equation becomes

$$\log. x = 3{\cdot}00507 - 0{\cdot}052664\, t + 0{\cdot}00000373\, t^2$$

2. The hard Bohemian, or potash-lime glass, density 2·431, experimented with, was 10 to 15 times a better conductor than the above ordinary glass. The resistance is found from the equation

$$\log. x = 1{\cdot}788 - 0{\cdot}04958\, t + 0{\cdot}0000711\, t^2$$

3. Flint, or potash-lead glass, density 2·983, has a resistance 1,000 to 1,500 greater than ordinary glass. It only begins to show signs of conductibility above 40° C.: at 46·2 its resistance is 6,182 million megohms; at 105°, 11·6. Its equation is

$$\log. x = 7{\cdot}2237 - 0{\cdot}088014\, t + 0{\cdot}00028072\, t^2$$

G. PLANTÉ—THE FORMATION OF THE LEAD PLATES OF SECONDARY BATTERIES.

(*Comptes Rendus, T.* 95, *No.* 9, 1882, *p.* 418.)

In the chemical actions which result in these batteries, the author remarked that a portion of the peroxide of lead was converted into sulphate of lead by the action of the dilute sulphuric acid; an observation which has been confirmed by the studies of Gladstone and Tribe. He sought to increase the storage capacity of the cells, by producing beforehand a galvanic deposit of lead on the electrodes, so as to allow of the oxidising action of the primary current penetrating more deeply into the substance of the lead plates. After trying unsuccessfully the effect of various lead compounds, the author came to the conclusion that the best method was to transform the substance of the plates themselves on the one into peroxide of lead, and on the other into reduced lead, by frequent changes in the direction of the charging current, alternating with periods of rest.

Admitting that only half the thickness of the plates is acted upon, and even that the peroxide is only reduced to protoxide, it would be quite reasonable to hope to obtain a discharge current sufficient for the deposition of 74 grammes of copper per kilogramme of lead in the secondary battery. (This would correspond to more than 222,000 coulombs.)

The system of reversals in the direction of the current, not only increases the successive layers of peroxide on one plate, but also transforms the other plate to a corresponding depth into lead galvanically reduced, so that, when the discharge takes place, whilst the hydrogen which results from the decomposition of the water reduces the peroxide formed by the primary current, the oxygen combines at the same time with an equivalent quantity of lead.

It is easy to see that the intervals of rest are beneficial for the formation of the plates, since when thus modified they are less good conductors. When they are again subjected to the action of the primary current, this current follows the line of least resistance: it does not pass to the liquid through the oxidised surface, but through the underlying metallic surface which is in contact with the liquid through the spongy upper layer. It thus oxidises a second layer of metal at the same time that it peroxidises the upper layer of protoxide.

This process requires a considerable time; but the author has found that the formation can be considerably expedited by raising the temperature of the liquid. This method, however, is attended with practical difficulties, and the preferable plan is to leave the plates for 24 to 48 hours immersed in a solution of nitric acid (half water, half acid). The action of the dilute nitric acid renders the plates more porous, and so develops a larger surface for the subsequent action of the current.

It has been found that plates thus subjected to the action of nitric acid, and then charged three or four times in opposite directions, can give in a week a current which could only be produced after several months' formation if the nitric acid treatment were not applied.

G. FOUSSEREAU—EFFECT OF TEMPERING ON THE ELECTRICAL RESISTANCE OF GLASS.

(*Comptes Rendus*, *T.* 96, *No.* 12, 1883, *p.* 785.)

The author made a series of observations on the above effect by the method described in a preceding abstract, using Lippmann's electrometer. The specimens used were in the form of small cups, cylindrical or slightly conical in shape, the two surfaces of which were in contact with the sulphuric acid in the apparatus. For temperatures higher than 80° C., mercury was used instead of the acid, as it was found that the fumes of the latter gave rise to a conducting film on the glass.

After a preliminary series of observations on the tempered specimen, it was buried in a sand bath, which was kept for a fixed time at any desired temperature, and the little glass cup was then allowed gradually to cool. When cooled, a second series of observations was made under the same conditions as the first. The time required to charge a condenser to a known difference of potential might then be taken as a measure of the resistance of the specimen.

The following results were arrived at:—

1. Tempering diminishes considerably the electrical resistance of different glasses.

This was most marked in the case of flint glass (potash-lead). Two specimens, one of which had been tempered and the other not, were heated together to the same temperature, and then allowed to cool, when it was found that the resistance of the specimen which had heen previously tempered had increased tenfold, while the resistance of the untempered specimen had hardly changed, thus clearly showing that the original tempering had considerably reduced the resistance.

2. A moderate reheating, which would only partially suffice to get rid of the elasticity due to the previous tempering, only partly destroys its action on the resistance.

3. The resistance of a piece of glass newly reheated continues to increase slowly during some length of time, as if it approximated to a definite state of equilibrium. The resistance of a piece of glass, whether it has been tempered or not, which has not been reheated, remains constant.

O. FRÖHLICH—THE APPARENT RESISTANCE OF THE ELECTRIC ARC.

(*Elektrotechnische Zeitschrift, B. IV., H.* 4, *April*, 1883, *p.* 150.)

The question whether the electric arc has a counter E.M.F. or not has been frequently raised, and attempts have been made to settle it experimentally. The author is not satisfied, however, that the existence of such an

E.M.F. has yet been demonstrated. It has also often been laid down that the difference of potential of the arc has a value which is nearly constant, and is about equal to 50 volts. That this law is not exactly correct appears at once, since, according to it, the electrical properties of the arc must be independent of its length. This, however, is contradicted by the fact that with a constant current the resistance of the arc increases with the length.

The resistance of the arc is the ratio of the difference of potential of the two carbons to the current; this may be termed the "apparent" resistance, and may include a counter E.M.F. The apparent resistance of the arc depends on the current and on the length of the arc, and a large number of experiments have been made by the author to determine this dependence.

It is difficult to find the law for the apparent resistance, since it rests on two independent variables. From the experiments a curve was constructed, and from this curve it appears certain that the difference of potential increases with the length of the arc, and that this increase is continuous.

If, therefore, S is the difference of potentials, L the length of the arc, and a b coefficients,

$$S = a + b \, . \, L. \quad \dots \dots \dots \dots \dots \dots \quad (1)$$

Or, if L is expressed in mm. and S in volts,

$$S = 39 + 1{\cdot}8 \, L.$$

The apparent resistance, R, of the arc is obtained by dividing S by the current: thus,

$$R = \frac{a}{I} + b \, . \, \frac{L}{I} \quad \dots \dots \dots \dots \dots \dots \quad (2)$$

or, in figures,

$$R = \frac{39}{I} + 1{\cdot}8 \, \frac{L}{I}$$

The values calculated from these formulæ agree very closely with the actual experimental values, as is shown in an exhaustive table.

The question arises whether from these formulæ it can be seen if the resistance of the arc is a true resistance, or of the nature of a polarisation. Let P be the counter E.M.F., or polarisation caused by the passage from the carbon to the arc, and from the arc to the other carbon, K the coefficient of conductivity, Q the cross-section of the arc; then

$$\left.\begin{aligned} S &= P + \frac{1}{K} \cdot \frac{L}{Q} \cdot I \\ R &= \frac{P}{I} + \frac{I}{K} \cdot \frac{L}{Q} \end{aligned}\right\} (a)$$

On the other hand, if U is the true resistance, then

$$\left.\begin{aligned} S &= \frac{U}{Q} I + \frac{1}{K} \cdot \frac{L}{Q} \cdot I \\ R &= \frac{U}{Q} + \frac{1}{K} \cdot \frac{L}{Q} \end{aligned}\right\} (b)$$

On comparing formulæ (a) and (b) with (1) and (2), derived from experiment, it is apparent that they can accord only on the hypothesis that the area of cross-section of the arc is proportional to the current. If this is accepted, and we put $Q = c\,I$, then we get

$$\left.\begin{aligned} S &= P + \frac{1}{Kc}L \\ R &= \frac{P}{I} + \frac{1}{Kc}\frac{L}{I} \end{aligned}\right\} (a^1)$$

$$\left.\begin{aligned} S &= \frac{U}{c} + \frac{1}{Kc}L \\ R &= \frac{U}{cI} + \frac{1}{Kc}\frac{L}{I} \end{aligned}\right\} (b^1)$$

equations which agree with (1) and (2).

To determine the coefficients a and b, it is evident that if we put $c = 1$, *i.e.*, the cross-section for unit current equal to unity, a may be regarded as the value of the polarisation, or of the resistance for unit cross-section, whilst b will be the reciprocal of the coefficient of conductivity of the arc.

From the experiments, however, it is not clear which hypothesis is correct, since both will satisfy the equations.

The difference of the appearances at the two carbons, and the fact that a fixed minimum E.M.F. is required to produce an arc, are in favour of the theory of polarisation; while, on the other hand, there is opposed to it the fact that the values found independently for such polarisation are about ten times greater than any other known form of polarisation.

From the equations (1) and (2) follows that the work done in the arc $W = S\,I = (a + b\,L)\,I$, and it is therefore proportional to the current, and therefore so also is the illuminating power.

L. SOHNCKE—ON THE MAXIMUM VALUE OF THE EFFICIENCY IN THE ELECTRICAL TRANSMISSION OF POWER.

(*Elektrotechnische Zeitschrift, B. IV., H. 4, April* 1883, *p.* 159.)

The work expended on the driven machine, and regained from the driving machine, are respectively, adopting Fröhlich's equations,

$$A_1 = c\,E_1\,I + p_1\,E_1^2$$

$$\text{and } A_2 = c\,E_2\,I - p_2\,E_2^2,$$

where A_1 and A_2 are expressed in horse-power, E_1 and E_2 in Daniells, and I by the ratio $\frac{\text{Daniell}}{\text{Siemens}}$ unit; while c is a constant which, according to Kohlrausch, has the value 0·00181, and p_1 and p_2 are the constants of the Foucault currents.

By introducing Ohm's law, $I = \frac{E_1 - E_2}{R}$, the above equations become

$$A_1 = c\left\{\left(\frac{1}{R} + K_1\right)E_1^2 - \frac{E_1\,E_2}{R}\right\}$$

$$A_2 = c\left\{\frac{E_1\,E_2}{R} - \left(\frac{1}{R} + K_2\right)E_2^2\right\}$$

where $K_1 = \frac{p_1}{c}$, and $K_2 = \frac{p_2}{c}$. Hence the efficiency will be

$$N = \frac{A_2}{A_1} = \frac{x - a_2 x^2}{a_1 - x}$$

where $x = \frac{E_2}{E_1}, a_1 = 1 + K_1 R, a_2 = 1 + K_2 R.$

This value of N is a maximum, when

$$x = \sqrt{\frac{a_1}{a_2}} \left(\sqrt{a_1 a_2} - \sqrt{a_1 a_2 - 1} \right)$$

and its value then will be

$$N_{max.} = \left(\sqrt{a_1 a_2} - \sqrt{a_1 a_2 - 1} \right)^2$$

If the constants K_1 and K_2 of the Foucault currents are alike for the two machines (*i.e.*, $K_1 = K_2 = K$) as is frequently the case

$$N_{max.} = \left(1 + KR - \sqrt{KR.}\ \sqrt{KR + 2} \right)^2$$

$$= \left(\sqrt{1 + \frac{KR}{2}} - \sqrt{\frac{KR}{2}} \right)^4$$

There is, however, a reason for the actual efficiency coming out higher than the value deduced from the formula, and this is the heating of the iron of the machine. The constant K is formed from a series of terms, the denominator of which contains the resistance of an iron wire, so that the specific resistance of iron always occurs in the denominator of K. This resistance increases with the temperature, hence K must decrease, and therefore the maximum efficiency is increased. Of course the rise in temperature causes an increase in the value of R, and therefore a decrease in $N_{max.}$; yet this decrease is always less than the increase of K, since the copper wire can cool more rapidly. The effect of the heating of the armature is not small. The specific resistance, w, of iron at the temperature t is

$$w = w^0 (1 + 0{\cdot}0042\, t);$$

if, then, w_1 is the specific resistance at the temperature $t + \tau$, then

$$\frac{w_1}{w} = 1 + \frac{0{\cdot}0042\, \tau}{1 + 0{\cdot}0042\, t}$$

From this value of w_1 would come out a lower value for K than if the value w were used in the calculation, and consequently a higher efficiency.

The value of the mechanical return can also be obtained from the above equation: it is

$$A_2 = \frac{c E_1^2}{R} \left\{ x - a_2 x^2 \right\}$$

This is a maximum when

$$x = \frac{1}{2a_2} \text{ and then } A_2 = \frac{C E_1^2}{4a_2 R}$$

In this case the efficiency

$$N = \frac{1}{2} \cdot \frac{1}{2 a_1 a_2 - 1}$$

$$= \frac{1}{2} \cdot \frac{1}{1 + 2(k_1 + k_2) R + 2 k_1 k_2 R^2}$$

and is thus less than $\frac{1}{2}$.

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The One Hundred and Twenty-third Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 10th, 1883—Mr. WILLOUGHBY SMITH, President, in the Chair.

The minutes of the last meeting were read and confirmed, and the names of new candidates announced and suspended.

Donations to the Library were announced as having been received from Mr. J. J. Fahie and Mr. W. T. Glover, and a vote of thanks was passed to those gentlemen for their presents.

The following paper was then read:—

ON THE ELECTRIC HOLOPHOTE COURSE-INDICATOR, FOR THE PREVENTION OF COLLISION AT SEA.

By Mr. J. H. A. MACDONALD, Q.C., Associate.

Mr. President and gentlemen,—I dare say you will all agree with me that there is no calamity which may be placed in the class of preventible accidents that is so destructive of property and of human life as collision at sea. And it is much to be feared that the risk of the occurrence of this terrible calamity is always on the increase. The courses which vessels have to steer, and the channels through which they are compelled to pass in entering or leaving port, are always becoming more and more crowded with shipping. The proportion of fast-going steam-ships to slow-going vessels is always on the increase. Everything that human ingenuity can devise for the purpose of increasing speed upon the sea is resorted to. Enormous seagoing steamers are now driven through the water at a speed which not very many years ago would have been considered impracticable even for light non-seagoing craft. The competition between companies for traffic, results in what is now known as "ocean racing," in which, both in the broad and in the narrow channels, the general rule is to put on the highest speed possible, but never to take off any of that speed unless there is an absolute necessity for doing so; and, indeed, sometimes considerable risk is taken rather than have any detention. The enormous size of vessels now adds greatly to the danger, as you will understand from several causes. In the first place, of course, a vessel 500 feet or 550 feet in length is not nearly so easily handled either in a narrow or in a wide channel as a small vessel. Then, again, a large vessel, from its extreme length, offers a much bigger target (if I may so express it) to anything that may tend to strike against it than a smaller vessel. Last and not least, the larger the vessel the greater the weight, and therefore the greater the certainty that if she has any way on her at all, and strikes anything, she will strike it with crushing force.

Returning to the question of speed for a moment, it would perhaps surprise some of the members present to know that vessels are now being propelled through the water at a velocity which within the memory of men now living would have been thought very extravagant for a railway train. I happened not very long ago to take up an old volume of the *Quarterly Review*, and found a very interesting and amusing article upon the subject of railways, praising them and stating of what use they would be to the commerce of the country and also to civilisation, and then the writer went on to say something like this: "But what shall we think of people who are mad enough to propose that railway trains should actually be run along the railroad at a speed of twenty miles or even thirty miles an hour?" And the writer goes on to express the most earnest hope that "Parliament in its wisdom would instantly suppress any such intention, and limit the speed of the railroad to twelve or fourteen miles an hour, and fix the utmost limit at twenty." Well, nowadays twenty-two miles an hour is not thought by any means an excessive speed to be obtained from a steam vessel at sea. You will see at once that two vessels going at that speed, whether approaching one another in a straight line or at an angle, diminish the distance between them at the rate of forty-four miles an hour, or one and a quarter minutes to the mile, or just about the speed of an ordinary express train. It is quite evident that with such a speed as that any mode of signalling by which one vessel can certify to another what her course is to be ought to have two characteristics: it ought, in the first place, to be rapid in giving the information, and, in the second place, it ought to be certain in giving the information.

Our present system is deficient in both of these particulars. I dare say you all know the present system perfectly well, which consists in placing coloured lights on each side of the vessel—a green light on the starboard side and a red light on the port side, with a board shutting off each light from the opposite side. It is quite plain that under such a system, which regulates itself, if an officer on the bridge of a steamer sees a coloured light at a distance of two miles, it is quite impossible for him to tell whether that coloured light will remain presented to him. What helm the vessel is on at that moment he cannot tell; but whatever course she may be upon at that time, if the officer in charge changes his course, that change will not be communicated until the vessel has come round sufficiently to shut out the one light and bring the other into view. Well, now, just take the present case of the two vessels approaching each other at a joint speed of forty-four miles an hour: if one vessel sights another at two miles off, and sees a coloured light, the time in which they will meet is exactly two and a half minutes, and you will at once see how absolutely necessary it is that any mode of signalling for the purpose of one vessel informing another of her course should be rapid and certain. This would be so, even if the curve of the arc described by a ship in paying off were invariably the same. But as the radius of the circle increases in proportion to the speed, while the time in which the circle is performed remains the same, it is plain that increased speed means increased danger, not only because there is less time for action, but because, the curves described being arcs of larger circles, the risk of their cutting one another is greater. [This was illustrated by a diagram.] We require, therefore, more power of opening communication with a coming vessel than we at present have, and we require more power of giving distinct information the moment we do open communication. Well, I think that electricity has supplied us with both the desiderata that we require. What we want is the means, by a powerful light, of intimating the approach of another vessel, and, the moment the presence of that vessel is intimated, giving information what course she is on and what course she intends to hold to. Now, an ordinary electric arc light gives us the very best means of opening communication, by early information of the presence of a vessel, because we can have great power in an arc light and can use it undimmed by coloured shades, which of course make the distance through which the light can be seen much less. Well, then, if by using that light in any way we can distinctly and clearly inform the other vessel what course we are on, undoubtedly that would be a great improvement upon the present system. But another thing is absolutely necessary, and that is that the operation should be so automatically controlled that there should be no conceivable possibility of a human mistake causing wrong information as to the course. Here, again, the convenient power we have in electricity, of producing a mechanical result at some distance from the point at which the human power puts the mechanical power into operation, enables us to effect this very easily; and my effort has been, in the instrument a model of which I am about to show you, to utilise these powers of electricity for the ends I have spoken of.

The instrument consists of a very strong reflector, and that reflector has an arc light placed in the middle of it. As long as the helm of the vessel is amidships, the reflector cannot be moved at all, but is held firm by two pegs, or detents. I am now pressing the handle of the reflector backwards and forwards, and, as you see, am unable to move it. But if the helm is moved from amidships, an electric circuit is formed, which actuates an electro-magnet, and thereby removes one of the detents. Let it be assumed that the helm is ported, then the reflector is set free by the withdrawal of one detent, so that by working the handle the light can be swept from amidships over the starboard bow—that is, over the side of the vessel to which she is paying off—and brought back again to midships. *Vice versa*, if the helm be starboarded, the reflector is freed from the other detent, so that the light can be swept from amidships over the port bow and back again. Now, plainly, that is not enough, because just as a man driving would give no practical direction to another driver by merely waving his hand back and forward, so merely waving the light back and forward would give no intelligible information. But if a driver waves his hand from left to right, and then drops it, not raising it till he has got it back to the left, and then again waves from left to right, he is at once understood. Now, to produce practically the same effect with the light, a screen is hinged in front of it, which, as the reflector is swung round, rises up and shuts out the light from view. The screen is held in position by a small catch while the reflector is being moved back to amidships, and at the instant it reaches that point the catch is pushed off, and the screen falling down exposes the light, which can then be again swung round, screened out, brought back, and re-exposed, as long as the helm remains in the same direction. The light is thus immovable when the helm is midships, and can be swept only over the starboard bow when the helm is ported—only over the port bow when the helm is starboarded. If the officer on deck signals to the other vessel, he cannot do so otherwise than in accordance with the helm.

The only other thing requiring to be provided against is the risk of the reflector being carelessly worked by not completing its sweep either way. For, of course, if the handle was not moved round far enough, the screen would not be raised up in front of the light; and, on the other hand, if its return swing was not completed, the screen would not fall off before the commencement of a new sweep. The instrument is therefore provided with two tell-tale bells, by which those on the bridge are able to check the working of the reflector, and make sure that it is being properly moved. One bell rings as the outward sweep is completed and the screen fixed; the other as the reflector comes amidships and the screen falls off.

There is one little improvement in this invention which I should mention, but which I am unable to show you to-night. You can quite understand that an instrument of this kind, and indeed any apparatus depending upon light, would be somewhat ineffective in foggy weather. The improvement in the arrangement I have made, to obviate the difficulty created by fogs, consists in using two steam-whistles which can be shunted into action by the reflector handle, one giving off a succession of short shrill notes, the other giving a succession of deep long notes; and this arrangement will enable the signals to be communicated in foggy weather, in supplement of the light, by keeping up a succession of shrill squeaking or deep groaning notes, according as the helm is to starboard or port.

Such, gentlemen, is the instrument which I have the pleasure of bringing to your notice to-night, which, although it is the invention of a mere camp-follower in the grand march on which your science has entered, has been most kindly and generously received. Should it prove in any degree useful, I shall be amply rewarded for any trouble I have had in devising it. But at least I shall always be grateful that it has been the means of introducing me to the association of your Society.

The President: All who have been down to the sea in ships will bear out what Mr. Macdonald has said. It is very desirable that there should be something of the kind adopted. I must say, as far as my experience goes, that signalling, generally speaking, of the class has not been so successful as I should have wished on our ships. Sailors, as a rule, have got great notions of keeping to their old system; they do not like any new-fangled systems; but I feel sure that the time is fast approaching when something of the kind will have to be adopted. I can only say that we are deeply indebted to Mr. Macdonald for his kindness in bringing the instrument here, and for the able way in which he has described it.

On the motion of the President, a unanimous vote of thanks was accorded Mr. Macdonald for his paper.

ELECTRO-MOTORS AND THEIR GOVERNMENT.

By Professors W. E. Ayrton, F.R.S., and John Perry, M.E.

The near approach of electric mains being laid under our streets, and the facilities they will offer for supplying electric power during the day time to factories, workshops, and even to private houses for domestic purposes, lends an immediate importance to considerations regarding the best form that should be given to electro-motors, as well as to the best conditions under which they should be used; while the comparatively novel question as to the methods that can be employed for governing motors, so as to make them run at the same speed whatever work they may be doing, is of equally pressing importance, seeing that this condition of constant speed must necessarily be fulfilled before we can expect to see lathes, circular saws, and other tools in our workshops regularly driven by electro-motors.

The great value of electro-motors arises—

1st. From their portability, seeing that they need only be connected by means of a pair of thin flexible wires with the main supply cables.

2nd. From their small weight per horse-power developed by them.

3rd. From the possibility of their being used at a considerable distance from the prime mover.

4th. From their working to the best advantage when run fast, and so being especially suitable for driving quick-moving machinery direct, without any intermediate gearing; for in a factory there is a great loss of power arising from the friction of a long line of shafting turning in its bearings, and, secondly, there is a loss of power in the intermediate gearing employed to make the drill on other quick-moving tool revolve much more rapidly than the shaft.

The portability of motors enables us to bring the machine-driven tool to the work, instead of, as at present, the work to the tool. For example, Fig. 1 shows a circular saw directly driven by one of our motors, and which can be moved about with tolerable convenience. Again, Fig. 2 shows an emery wheel also driven by one of our motors, and which can be moved about for the purpose of polishing any large piece of metal.

The third and fourth advantages make electro-motors of especial importance to us Londoners at this present moment.

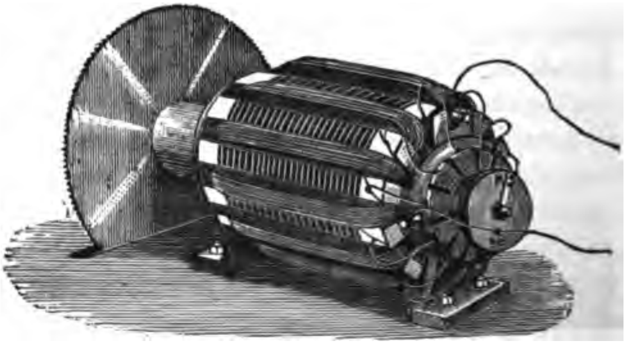

Fig. 1.

The unsightly blow-holes on the Thames Embankment and at this end of Victoria Street have now fallen out of favour, if they ever were in favour. Although English people are not supposed to be strong in matters of taste, we still cannot contemplate with equanimity a series of low chimneys, emitting foul gases, scattered along our principal thoroughfares, especially when perfect ventilation can be obtained by fans directly driven by motors. Such a fan, kindly placed at our disposal by Rev. Mr. Capell, is shown in Fig. 3, three feet in diameter, which is now passing 16,000 cubic feet per minute at 1,000 revolutions, and is driven by one of our

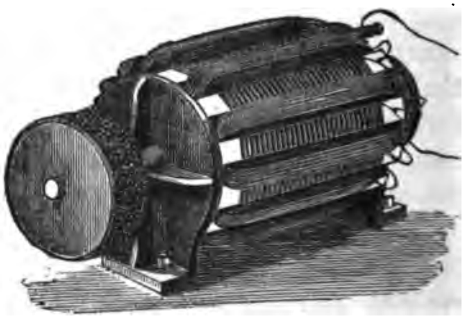

Fig. 2.

motors, the electricity being supplied by Faure-Sellon-Volckmar accumulators, kindly lent us by the Electrical Power Storage Company. These fans might be stationed along the tunnel, each driven directly by a motor without intermediate gearing. A single going and return insulated wire would convey an amount of power which seems incredible to a mechanical engineer; and a steam-engine doing the work, as well as a dynamo-machine supplying the electricity, might be all in some backyard, out of everybody's way. The gas might be sent into the water of the Thames, and, if even the bubbling up of the gas was thought objectionable, it might bubble up in recesses in the walls, so that the bubbles could not be seen. The particular specimen of our electro-motors which we are employing this evening has adjustable lead and reversing gear: this, of course, would not be necessary for motors permanently fitted and always run at a fixed speed, but it is very handy for motors when used experimentally. The details of the reversing gear can be seen from figures which come later on in this paper, where a description of the gearing will be found.

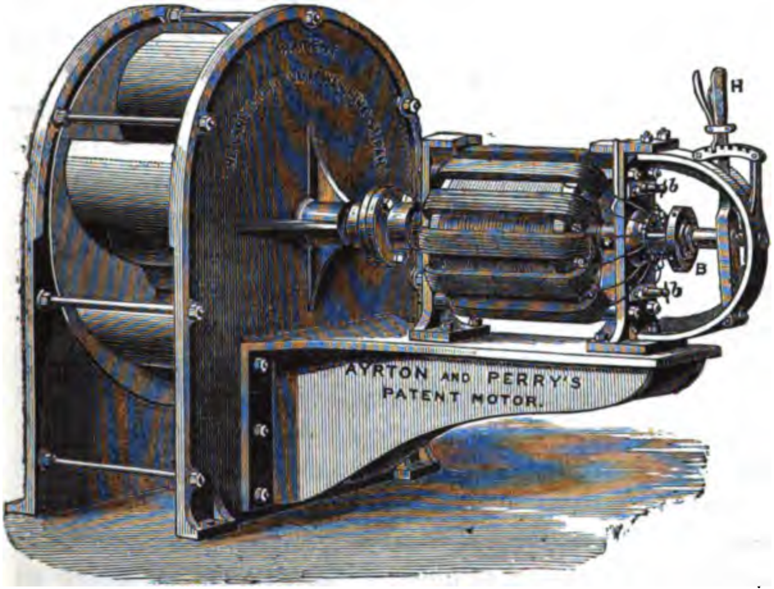

FIG. 3.

The smaller 2-foot fan, which is being driven also by one of our motors, is passing about half the quantity of air.

The portability of electro-motors and their light weight make them of enormous importance where locomotion is involved. It may be in the memory of some of those present that a year ago we advocated the automatic electric transport of goods and passengers by means of electric railways without engine-driver, guard, or signalman, and we showed experimentally that the idea was perfectly feasible when our absolute block-system was employed. This idea has borne fruit, for Professor Fleeming Jenkin, with his usual enthusiasm, was fired with it, and our conjoint ideas have led to what is known as "telpherage," the meaning of which can be gathered from the words "telegraph" and "phosphoros." "Telepherage" the Greek scholars among you will no doubt consider better, but in these days of speed and brevity, when the words "ergs," "volts," and "ohms" are tolerated, the abbreviation of "telepherage" to "telpherage" will doubtless be pardoned. We may mention that telpherage experiments on a working scale are on the eve of being tried, so that we hope that "to telpher" will shortly be a common English expression.

For telpherage, and indeed for motor work generally, it became of immense importance to know what horse-power per pound dead weight of motor could be practically obtained, what efficiency could be relied on, and how a motor could be best governed that is always made to go at the same speed whatever work it was doing. We have therefore been engaged on a long series of experiments on motors of different sizes, but before giving an account of the result of these experiments we will consider theoretically the general principles involved in the government of motors, and exhibit one of our motors which is governed without using anything of the nature of a mechanical governor.

GOVERNING.

A motor may be governed to run at the same speed whatever work it may be doing, in one or other of two distinct ways. 1st, the power may be cut off in proportion as the work is cut off; or, 2nd, a brake may be employed to replace the useful load removed, which brake may either waste the excess power or store it up for future use. Hitherto the first method alone has been attempted, viz., that of cutting off the power, and usually with this very serious defect, that instead of the power cut off being directly in proportion to the work cut off, the arrangements have been such that either all power was cut off or none, so that the motion of the motor was spasmodic, just as in an ordinary gas-engine, which suffers from the same defects, that full charge of gas or no charge are the usual only alternatives. A sample of an electro-motor governor of this type, and which may be called a "spasmodic governor," is shown (Fig. 4), and consists merely of a rotating mercury cup into which dips a wire, which makes in this case contact with the mercury, and so completes the circuit when the speed is slow, but which, on account of the hyperbolic form assumed by the surface of the mercury as the speed rises, ceases to dip into the mercury at high speeds, and so breaks contact. This governor, you observe, is very erratic in its action, yet it will be found among the modern specifications at the Patent Office.

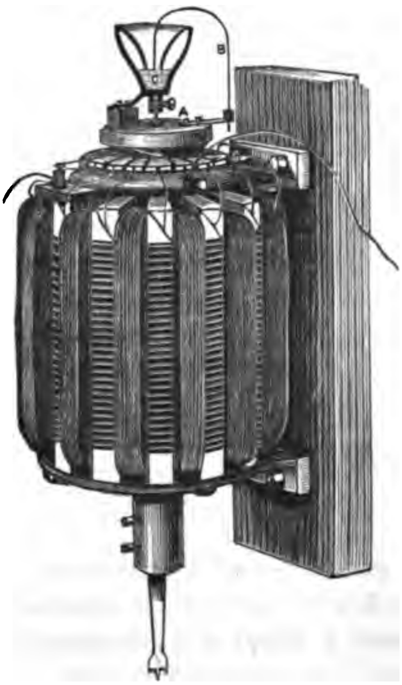

Fig. 4.

The first improvement we made in governing consisted in replacing the "spasmodic governor" by a "periodic governor." With our "periodic governor" the power is never cut off entirely for any length of time; nor, on the other hand, is full power ever put on for any length of time, but in every revolution power is supplied during a portion of the revolution, the proportion of the time in every revolution during which much power is supplied to the time during which less is supplied depending on the amount of work the motor is doing. Our periodic governor, then, differs from the spasmodic governor in the same way that a good loaded steam-engine governor differs from the ordinary governor of a gas-engine. One of the ways of effecting this result is as follows:—A brush, A, Fig. 5, lies on the rotating-piece, B K, the cylindric surface of which is formed of two conducting portions connected with one another through any resistance, and the brush, A, is moved along the cylinder, B K, under the action of the governor balls. When the brush, A, is touching the contact part, B, the motor is receiving current directly; but when A rests on the part K the motor receives current through the resistance which is interposed between B and K. If the governor balls fly out, the brush is moved along B K, so that there is contact with K during a greater part of the revolution than before; and if the governor balls come together, the speed of the motor being too small, the brush is moved in the opposite direction, so that it makes contact with B for a longer time during each revolution.

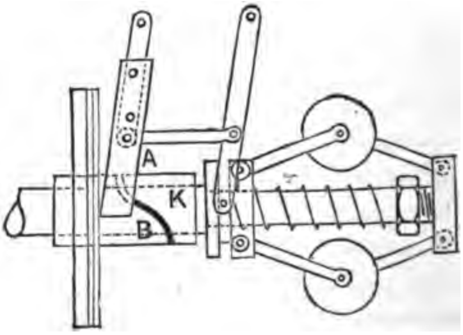

FIG. 5.

If the motors are in series, we arrange that the periodic governor shunts the currents periodically, instead of introducing resistance. In this case the connections are as follows:—

B is made of wood, while K is made of metal. K is connected to one end of a shunt coil, the other end of the shunt being connected to one of the terminals of the motor and A is connected to the other terminal of the motor. If, then, A rests on B, the shunt is inoperative, and all the current passes through the motor; whereas, if A rests on K, the shunt is in operation, and part of the current only passes through the motor.

We are fitting up one of our periodic governors to a Gramme machine working as a motor in series with accumulators —that is to say, a dynamo machine is employed to work the Gramme machine as a motor, and to charge the accumulators. When the motor is running too fast, all the current is, during the greater part of each revolution, shunted past the motor through the accumulators, and the current only passes through the motor during a small portion of each revolution; whereas, when the motor is going more slowly, all the current passes through the motor during the greater part of each revolution before passing through the accumulators.

It is difficult with any such governors to entirely prevent sparking, and hence we prefer a totally different form of governor, that we have devised, depending on the second system referred to above—that is to say, the employment of a brake, which brake stores up the superfluous power supplied to the motor. It is well known that if a dynamo machine be running empty, and its poles be closed, a great backward pull is exerted on the driving mechanism, in consequence of the dynamo machine now commencing to send a current and to do work; but not only will the closing of the circuit of a dynamo machine cause a pull to be put on the driving mechanism, but even a slight increase in the speed of a dynamo, the circuit of which remains unaltered, will also produce the same effect if the speed be the critical speed for the resistance through which the dynamo is sending a current, because this critical speed is that below which but a small current is sent by the dynamo machine, and above which a great current is sent by the dynamo machine—that is to say, below the critical speed for any particular resistance the dynamo acts very feebly, while above that speed it acts as a powerful dynamo machine. Such a dynamo machine, then, with closed circuit, attached to the motor and driven by it, would act as a brake, and, if the normal speed of the motor was the critical speed of the dynamo for the particular resistance in its circuit, it would act as a very efficient brake, causing a great opposition to even a small increase of speed beyond that at which it was desired the motor should run at.

Hitherto we have spoken of the dynamo brake as if the electric power produced by it were wasted, but if it be in the same circuit as the motor, then the electric power produced by its rotation will help on the current, and the energy it absorbs in acting as a brake will not be subtracted from the energy of the whole system, but will merely diminish the speed of the particular motor in question. Fig. 6 shows such an arrangement, in which a magneto-motor is geared with a small series dynamo—that is to say, the same spindle carries the armatures of both machines. The current passes through both the armatures in series, and then through the field-magnet of the motor. In consequence of its passing through the armature, *m*, of the motor, the spindle is rotated, and a back electromotive force is set up, resisting the current, whereas the motion of the armature, *d*, of the dynamo sets up a forward electro-motive force, and assists the current.

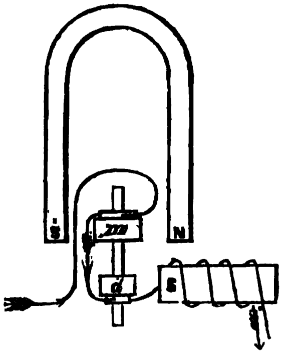

Fig. 6.

This combination of motor and generator brake is the gist of our method, and the exact combination we employ in any particular case depends on what are the supply conditions—that is to say, whether constant difference of potential is kept up at the terminals of the motor, or constant current is passing through the motors. When motors and lamps are in parallel circuit, then, in order that any one of them shall operate independently of all the others, it is necessary that constant difference of potentials shall be maintained between the supply leads; whereas, if they are in series, it is the current that must be kept constant to make the one apparatus which is consuming the electric energy independent in its action of the remainder. We have elsewhere described in detail the various methods we employ for obtaining constancy in the supply conditions in each of these two cases, and we shall assume in this question of governing motors that one or other of these supply conditions is kept constant.

1st. *When constant potential difference is maintained between the main supply leads.*

Fig. 6 is one of the arrangements that might be employed for governing the motor, and, although suitable enough for stationary motors would have the disadvantage, if the motor had to be used for locomotion, that it contained two distinct machines. In practice, then, we prefer to replace the arrangement shown in Fig. 6 by that shown in Fig. 7 or 8.

In Fig. 7 we have a magneto-motor, but, in addition, the wire forming part of the main circuit is coiled round the limbs of the permanent magnet in such a direction that the current passing through it weakens this permanent magnet. The result is that the armature and permanent magnet alone form a motor, whereas the armature and the coils of wire wound round the permanent magnet form a brake-dynamo. The current passing through the armature, acted on by the permanent magnet, produces motion, and hence is resisted, while the current passing through the armature and through the coils wound round the permanent magnet resists motion, and hence is assisted.

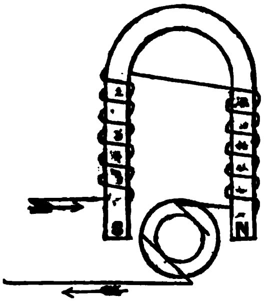

FIG. 7.

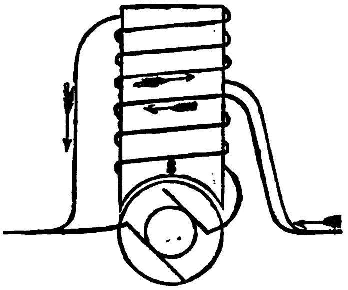

FIG. 8.

Fig. 8 differs only from Fig. 7 in that the permanent magnet is replaced by a shunt circuit, which it is easy to see it is possible to do if it be remembered that the supply condition we are dealing with is "constant difference of potentials at the terminals of the motor;" hence the current which passes through the shunt circuit will be a constant current, and the iron of the field-magnet, as far as this circuit is concerned, will act as a permanent magnet. The current then passing through the shunt circuit magnetises the field-magnet, as shown in the figure, but, in addition, the wire which forms part of the armature is wound round this field-magnet in such a way that the current passing through it tends to demagnetise the field-magnet, and here, as before, the armature and shunt circuit act as a motor, while the armature and the thick wire coil, which is a continuation of the armature, and which is wrapped round the field-magnet, form a brake-dynamo.

So far we have been dealing with motors in parallel circuit, with constant difference of potentials at their terminals. We will proceed to consider motors in series, and in that case the supply conditions will be—

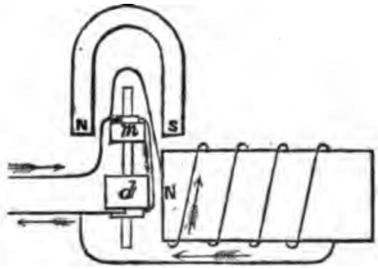

FIG. 9.

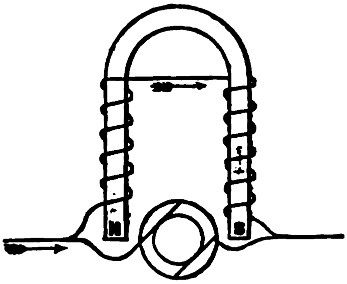

FIG. 10.

2nd. *Constant current passing through all the motors.*

Here we may also combine two armatures working on a common spindle, as shown in Fig. 9, but in this case the magneto machine is a brake-generator, while the motor, which is the larger, is a shunt-motor, the field-magnet being a shunt to both armatures, which latter are in series.

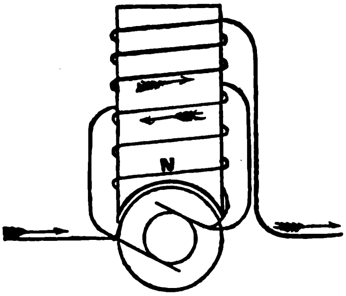

FIG. 11.

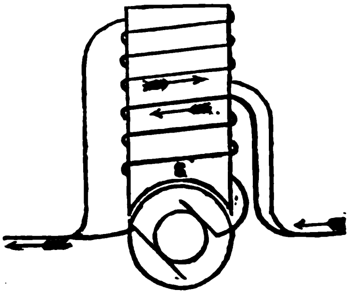

FIG. 12.

This double machine may be replaced by a coiled magneto-machine, as in Fig. 10, in which we have a magneto-generator acting as a brake, and a shunt motor. Or, again, this may be replaced by the arrangement in Fig. 11, where the fine wire coil, which is a shunt to the armature only, acts with the armature as a motor; and the thick wire coil, which forms part of the main circuit, constitutes, with the armature, a brake-generator, so that the main current passing round this thick wire coil tends to demagnetise the field-magnet. Or, again, the shunt coil, which with the armature constitutes the motor, may be a shunt to both the armature and the demagnetising coil, as shown in Fig. 12.

The subject of governing motors may be mathematically investigated as follows:—

1. *When constant potential difference is maintained between the mains feeding motors in parallel circuit.*

Let the arrangement shown in Fig. 7 be that adopted, and consisting of a magneto-motor and a series brake-dynamo.

Let e be the back electro-motive force set up in the motor when n is the number of revolutions per minute at which it is turning; let C be the current passing through it, then we know that

$$e = n\,(a - \mathrm{K\,C}),$$

where a is a constant depending on the permanent magnetism, and K on the coiling and the magnetic susceptibility of the steel magnet. But if E is the potential difference constantly maintained at the terminals of the motor, and R is the resistance of the armature and demagnetising coil combined, then

$$e = \mathrm{E} - \mathrm{R\,C},$$

but the total work the motor can do in consequence of its motion is

$$e\,\mathrm{C} = n\,(a - \mathrm{K\,C})\,\mathrm{C} = (\mathrm{E} - \mathrm{R\,C})\,\mathrm{C},$$

which we desire to be able to vary at will without varying n. The second equation is therefore true for all values of C, which is the only variable, hence we must have

$$n\,a = \mathrm{E},$$
$$\text{and}\quad n\,\mathrm{K} = \mathrm{R}.$$

But if we consider the dynamo part alone, the approximate equation for the early part of its characteristic curve would be of the form

$$e_1 = n\,\mathrm{K\,C}.$$

Consequently, since one of the conditions for governing is

$$n\,\mathrm{K} = \mathrm{R},$$

we conclude that the resistance of the armature and demagnetising coil combined is such that it is the critical resistance of the dynamo part of the combination for that speed at which it is desired the motor shall always run, no matter what amount of work it may be doing.

To better explain this, we will take an actual case and show how we experimentally determine the proper resistance to give to the governing demagnetising coil for any particular motor when the arrangement shown in Fig. 8 is employed.

Let f_1 be the resistance of the field-magnet of a series motor of any size and type, and a_1 the resistance of the armature. Pass a current, C_1, through the field-magnet only, and let e_1 be the electro-motive force set up in the armature when it is rotated mechanically at a speed of n_1 revolutions per minute. If there be permanent magnetism in the field-magnet, then two experiments must be made, in one of which the permanent magnetism of the field-magnet alone acts, while in the second both the permanent magnetism and the magnetism due to the current C_1 are combined, and e_1 then stands for the difference between the electro-motive forces set up in the two experiments—that is, e_1 is the electro-motive force due to the total magnetism produced by the current alone.

Then if the field-magnet of the motor be rewound with two coils, one occupying $\frac{1}{x}$ of its volume, and having a resistance f_2 and the other $\frac{1}{y}$ of an equally useful part of its volume, and having a resistance f_3, and if a current, c, passes through the former, and C through the latter, the electro-motive force, e_2, set up in the armature coil, which, if rewound, may have a resistance a_2, will at n_2 revolutions per minute be

$$e_2 = \frac{e_1}{n_1 \, C_1 \sqrt{a_1 f_1}} \, n_2 \sqrt{a_2} \left(p + c \sqrt{\frac{f_2}{x}} + C \sqrt{\frac{f_3}{y}}\right)$$

where p is the term due to the permanent magnetism which by definition is inoperative as far as e_1 is concerned, but is effective in producing e_2.

If the coil of resistance f_3, which is a continuation of the wire on the armature, as shown symbolically in Fig. 13, be so wound as to diminish the total magnetism in the field-magnet, the above equation reduces itself to the form

$$e_2 = n_2 (s + tc - \mathrm{K\,C});$$

and, reasoning as before, we obtain for constant speed, independent of the work the motor may be doing, the conditions

$$n_2 (s + tc) = \mathrm{E}$$

$$n_2 \mathrm{K} = a_2 + f_3,$$

where f_3 is the resistance of the regulating coil.

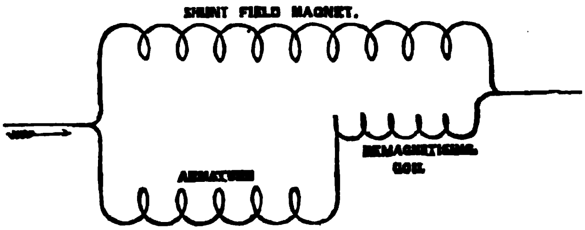

FIG. 18.

To determine f_3, we have

$$f_3 = n_2 \mathrm{K} - a_2$$

$$\text{and} \quad \mathrm{K} = \frac{e_1}{n_1 \mathrm{C}_1 \sqrt{a_1 f_1}} \sqrt{\frac{a_2 f_3}{y}}$$

$$\therefore f_3 - \frac{n_2 e_1}{n_1 \mathrm{C}_1 \sqrt{a_1 f_1}} \sqrt{\frac{a_2 f_3}{y}} + a_2 = 0$$

$$\therefore \sqrt{f_3} = \frac{\frac{n_2 e_1}{n_1 \mathrm{C}_1} \sqrt{\frac{a_2}{a_1 f_1 y}} + \sqrt{\frac{n_2^2 e_1^2}{n_1^2 \mathrm{C}_1^2} \frac{a_2}{a_1 f_1 y} - 4 a_2}}{2}$$

a_1, f_1 are the resistances of the armature and field-magnet of the motor as received for trial, and are therefore known. e_1 is the electro-motive force the motor sets up when it is revolved at n_1 revolutions per minute, and a current, C_1, sent through its field-magnet; e_1, n_1, and C_1 are therefore determined experimentally. y and a_2 depend on the rewinding; a_2 may be made equal to a_1 if we like—that is to say, the armature need not be rewound; y must be chosen judiciously, and its value is governed by the following considerations:—The presence of the governing coil diminishes the power of the motor, except when it is exerting its maximum power—not, however, by wasting the power, but by making the motor for the time being equivalent only to a smaller motor. Any form of governing an electric or any other motor must necessarily have this effect: for example, when a 20 horse-power steam-engine is only turning the shafting of a factory, and doing no other work, the steam may be so much cut off by the governor that for the time being the engine may only be exerting 2 horse-power, which quite a small engine could of course just as well exert. As a matter of economy in weight, etc., it is not of course desirable to use a motor which is very much more powerful than is necessary, or, in other words, it is not desirable that the amount of governing should be necessarily very large; hence $\frac{1}{y}$ should not be too large. On the other hand, $\frac{1}{y}$ must not be too small, or the whole governing action will be too small, corresponding with a steam-engine the governor of which has only a very limited range. Probably $\frac{1}{y} = \frac{1}{5}$ and $\frac{1}{x} = \frac{4}{5}$,—that is, $y = 5$ and $x = \frac{5}{4}$ will generally give very good results,—and those are the values we have employed in the one of our motors before you, which is thus governed by the combination of a shunt motor with a series brake-dynamo. $a_1, f_1, e_1, n_1, C_1, a_2$ and y having been thus determined, n_2 merely depends on the speed at which we desire the motor always to run, so that f_2, the resistance of the regulating coil, can now be found.

We will next consider the case

2. *When constant current is maintained through the motors.*

In Fig. 11 let a and f be the resistances of the armature and field-magnet respectively, A and F the currents passing through the armature and field-magnet, and C the total current which by the supply conditions is kept constant; then, if e is the back electromotive force produced when the motor is running at n revolutions per minute,

$$e = n\,(p + q\,\mathrm{F} - \mathrm{K}\,\mathrm{C}),$$

where p is a constant depending on the permanent magnetism in the field-magnet, q on the winding of the shunt coil, and K on the winding of the demagnetising coil on the field-magnet.

But $$e = f\,\mathrm{F} - a\,\mathrm{A},$$

$\therefore$ the total work the motor can do in consequence of its motion equals

$$e\,\mathrm{A} = \mathrm{A}\,n\,(p + q\,\mathrm{F} - \mathrm{K}\,\mathrm{C}),$$
$$= \mathrm{A}\,(f\,\mathrm{F} - a\,\mathrm{A}).$$

But $$\mathrm{F} + \mathrm{A} = \mathrm{C},$$

therefore, eleminating F, we have the total work equal to

$$\mathrm{A}\,n\,\{p + q\,(\mathrm{C} - \mathrm{A}) - \mathrm{K}\,\mathrm{C}\} = \mathrm{A}\,\{f\,(\mathrm{C} - \mathrm{A}) - a\,\mathrm{A}\},$$

which we desire to be able to vary at will without varying n. Hence, as this equation must be true for all values of A, we must have the coefficients of A equal, as well as those of A^2, so that

$$f\,\mathrm{C} = n\,\{p + (q - \mathrm{K})\,\mathrm{C}\}$$
$$f + a = n\,q$$

are the conditions for governing when constant current is supplied to the motors. And the second of these means that the normal speed of the motor must be the critical speed for the motor when used as a shunt dynamo with an infinite resistance external to the machine.

If the shunt coil be a shunt to both the armature and the demagnetising coil, as shown in Figs. 12 and 13, then the equations become

$$e = n\,(p + q\,\mathrm{F} - \mathrm{K}\,\mathrm{A})$$
$$= f\,\mathrm{F} - (a + r)\,\mathrm{A}.$$

where r is the resistance of the demagnetising coil. Now, the first equation may be put into the form

$$e = n\,\{p + (q + \mathrm{K})\,\mathrm{F} - \mathrm{K}\,\mathrm{C}\},$$

therefore these equations are exactly like the two employed in the previous case, except that $q + \mathrm{K}$ replaces q, and $a + r$ replaces a. Consequently, if these changes be made in the former equations for governing, we shall have the equations for governing applicable to this second case, which are, therefore,

$$f\,\mathrm{C} = n\,(p + q\,\mathrm{C})$$
$$f + a + r = n\,(q + \mathrm{K}).$$

And this second equation means that the speed at which it is desired the motor shall always run, independently of whatever work it may be doing, is the critical speed for the motor when used as a shunt dynamo when the resistance external to the motor is infinite.

In both these cases, therefore, for constant current, *n* the normal speed of the motor is the speed at which the motor will begin to feed itself when used as a shunt dynamo, the external circuit being broken, but the connections in the motor itself left untouched.

In working out this theory we have, for simplicity, assumed—what is generally true in dynamos—that the magnetic field is mainly due to the action of the field-magnet. We will presently show why this is not the case in our motor, and why we believe it should not be the case in any good motor. But, although the difference in construction is very considerable, the formula employed in the mathematical investigation of the conditions for governing are similar to those we have employed, and consequently we have not thought it desirable to lengthen our paper by inserting them.

In the preceding we have also, for simplicity of calculation, assumed that the strength of the magnetic field produced by an electro-magnet is proportioned to the current passing round it, but this is of course only true when the magnet is far from saturation. A function, to express more accurately the strength of the field, must of course be such that for all very strong currents the field has a constant strength. Five years ago, Mr. Schwendler deduced, from his experiments on dynamo machines made at the India Store Department, that the equation connecting the electro-motive force of a series dynamo, running at a particular speed, with the internal and external resistances was of the form

$$E = K\left\{1 - E^{-a\left(\frac{r}{r'}\right)^2}\right\}$$

where r is the resistance of the machine, r' the external resistance in circuit, and K and a constants. And one of us who had the good fortune to be assisting Mr. Schwendler in these investigations, in 1878, made at the time a large number of calculations to determine the most probable value of the constants in this function. Having, however, previously (when engaged with our students in Japan in putting into a convenient form for calculation some of the results given in Wiedemann's "Galvanismus," for the strength of an electro-magnet) employed the expression $\frac{k\,C}{1 + s\,C}$ for the strength of the magnetic field produced by a current, C, flowing round an electro-magnet, we saw that the equation for the curve connecting the electro-motive force of a dynamo with the current flowing through its coils could be expressed in the form

$$E = n\left(p + \frac{k\,C}{1 + s\,C}\right),$$

where n is the speed, and p a constant depending on the permanent magnetism in the field-magnets; and this formula, which it would naturally strike any practical mathematician to employ, we used then, and have ever since used in our calculations on dynamo machines and motors, and in our lectures to students. From this equation we were led theoretically, at the commencement of 1879, to the idea of critical speed in a dynamo; but finding shortly afterwards that the idea of a critical speed had been foreseen some years previously by Sir William Thomson, and that it was already referred to in the reprint of his papers on electricity and magnetism, we did not think it necessary to refer publicly to the results we had arrived at by the use of the above equation. We, however, now take this opportunity of alluding to the use of this equation for expressing the total electro-motive force for a dynamo in terms of the current flowing through its coils, since in France, Germany, and England this equation has comparatively recently given rise to various claims as to priority of employment.

Another formula which we have also for some years been in the habit of using to express the strength of an electro-magnet in terms of the current flowing through its coils is,

$$a \tan.^{-1} b\,C,$$

but this does not lend itself so readily as the preceding to algebraical manipulation or the theory of dynamo machines.

In the preceding investigation for governing motors we have, for simplicity, assumed that the resistance of the armature was a constant for a given temperature, but in reality this resistance depends on the number of reversals of magnetism in the coils of the armature per minute—that is, depends on the speed of rotation of the motor. That the resistance of the armature was not a constant was shown by M. Cabenallas in 1880,* but we are not aware that much attention has been paid to this very important fact in theoretical investigations of dynamo machines or motors, or that any attempt has been made to ascertain exactly how this increase of resistance depends on the speed of rotation and on the coefficient of self-induction. The calculation is comparatively simple, and is as follows:—

INCREASE OF RESISTANCE OF THE ARMATURE DUE TO SELF-INDUCTION.

Let M′, Fig. 14, represent, symbolically, a coil just on the point of being short-circuited by the brush A. On the armature moving in the direction of the upper arrow, as is the case in an ordinary dynamo or motor, or on the brush moving in the direction of the lower arrow, the armature being fixed, as is the case in our motor, the coil M, which previously had no current flowing in it, will now have a current, $\frac{C}{2}$, started in it, whereas the coil M′, which just previously had a current $\frac{C}{2}$, flowing in it, will

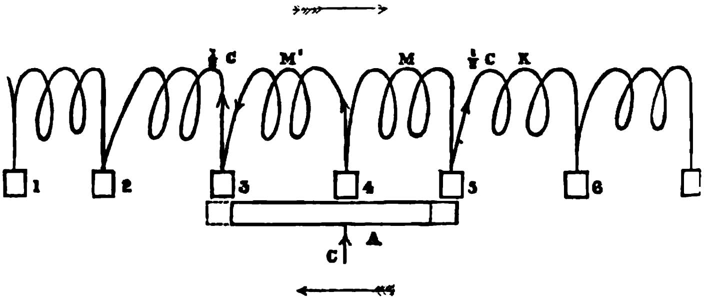

FIG. 14.

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* *Comptes Rendus*, Tome 90, No. 23, June 7th, 1880.

be short-circuited. The result of this will be that, even if the brush be exactly in a neutral part of the field, so that there is no electro-motive force in a short-circuited coil due to its cutting lines of force, still a certain amount of energy will be expended in starting the current $\frac{C}{2}$ in the coil M, and an exactly equal amount of energy will be wasted in the coil M′ by the strong current momentarily induced in it on short-circuiting.

If l is the coefficient of self-induction of the coil M, then the back electro-motive force in it, due to starting the current $\frac{C}{2}$ in it, will be $\frac{1}{2}\,l\,\frac{dC}{dt}$, and the amount of energy thus expended, to be wasted when this coil is short-circuited at the next brush, is $\frac{1}{2}\,l\left(\frac{C}{2}\right)^2$ or $\frac{1}{8}\,l\,C^2$. Hence, the waste of energy due to one coil passing a brush, however many coils there may be short-circuited at a brush at any one time, is $\frac{1}{8}\,l\,C^2$, if the brush be in the neutral part of the field; or, if it be remembered that on a coil passing one brush, another coil symmetrically on the opposite side of the armature passes the other brush, the total loss of energy produced by the coil rotating through a circumferential distance equal to the breadth of one coil is $\frac{1}{4}\,l\,C^2$. Hence, if L is the coefficient of self-induction per radian of the coil, and if n is the number of revolutions per minute, the loss of energy per second is

$$\frac{1}{4}\,L\,C^2 \times \frac{2\,\pi\,n}{60},$$

since $\frac{2\,\pi\,n}{60}$ is the angle turned through per second.

The waste of energy per second is consequently $\frac{\pi\,n\,L\,C^2}{120}$, and this corresponds with an increase in the resistance of the armature equal to $\frac{\pi\,n\,L}{120}$ ohms.*

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* Since presenting this paper we have learnt that a somewhat similar calculation has been recently made by M. Joubert, and his calculation will be found in the *Comptes Rendus*, Tome 96, No. 10, for March 5th of this year; but M. Joubert's result is not expressed in the simple form of an increased resistance of the armature as we have here expressed it.

The following gives a sample of the results our students have obtained when actually measuring the resistance of a fine wire Gramme ring rotating at different speeds in space at a distance from any iron except the iron wire composing its core, and traversed by a current which was kept constant in strength to avoid any change in the static resistance arising from changes in the heating of the wire by the passage of the current:—

Number of revolutions per minute.	Current in ampères traversing the Gramme ring.	Approximate		
		Resistance in ohms.	Increase of resistance.	Equivalent back E.M.F. in volts.
0	·00488	1,768	...	...
670	"	1,800	32	·156
825	"	1,810	42	·209
1,050	"	1,900	132	·642
1,300	"	1,920	152	·744
1,490	"	1,995	227	1·15
1,770	"	2,060	292	1·42
2,230	"	2,200	432	2·12

A number of sets of experiments similar to the above were made, and all led to the same result—that the resistance increases more rapidly than the speed, instead of the increase being proportional to the speed, as might have been expected from our reasoning on self-induction just given. Since the reading of the paper, however, the main cause of this apparent discrepancy between theory and practice has been discovered, and is as follows:—In order to eliminate the effect of the earth's magnetism setting up an electro-motive force in the rotating bobbin, the brushes were so placed on the commutator that when the bobbin was rotated at a particular speed no electro-motive force was set up due to the earth. But in reality such an adjustment of the brushes must be made for every speed, unless the axis of rotation of the bobbin is that of the dip, since the magnetic poles induced in the iron of the bobbin are carried forward by the rotation, and are carried forward the more the greater the speed. In spite, therefore, of the careful adjustment of the brushes in obtaining the results, a sample of which is given above, it was only at one particular speed that there was no electro-motive force due to the earth: at all speeds below this the electro-motive force was assisting the current used for testing, and at all speeds above this it was opposing this current, thus making the increase of resistance too small at low speeds, and too large at the high.

To eliminate the error produced by shifting of the poles by rotation, without resorting to the very troublesome operation of adjusting the brushes for each speed, we have in all our later experiments taken readings with both a positive and a negative current at each speed, and the following is a sample of the more accurate results thus obtained, the resistance of the Gramme ring at rest being 1,860 ohms :—

Revolutions per minute.	Increase of resistance		Mean increase of resistance.	Increase of resistance calculated from $\frac{\pi n L}{120}$
	+ Current.	— Current.		
600	70	50	60	75
800	110	75	92	100
1,000	150	90	120	125
1,205	200	110	155	150
1,400	245	130	187	175
1,602	320	120	220	200
1,806	405	150	277	212
2,000	480	190	335	250

Here the increase of resistance is far more nearly proportional to the speed than before, and, what is still more interesting, is approximately what is obtained by using the formula $\frac{\pi n L}{120}$, given above, up to speeds of about 1,600 revolutions per minute. For experiments made to determine the coefficient of self-induction by the Wheatstone's bridge method, described by Clerk Maxwell, showed that it was about 7·5 ohms. The last column above shows the increase of resistance calculated from the expression $\frac{\pi n L}{120}$, using this value for L.

In addition to this increase of resistance due to self-induction, there is, as is explained further on, a back electro-motive force set-up by the magnetic poles formed in the Gramme ring by the testing current not being at the brushes themselves, but, instead, carried forward by the rotation; and it is possible that this electromotive force, not being proportional to the speed, is the reason why the total increase in the resistance is not strictly proportional to speed, or it may be that at the very high speeds there is a slight jumping of the brushes. The real cause we are now endeavouring to ascertain experimentally.

In the preceding we have spoken of the armature as moving in ordinary motors, and the armature as fixed in ours. To make this quite clear, we may mention that we define the "armature," whether movable or fixed, of a continuous current dynamo or motor as that part of the machine in which the current is being perpetually reversed as the machine works; and the "field-magnet," whether fixed or movable, as that part round which the current is always flowing in the same direction.

RESULTANT MAGNETIC FIELD, AND THE PROPER LEAD TO GIVE TO THE BRUSHES IN A DYNAMO AND IN A MOTOR.

In an actual dynamo machine or motor, in addition to this practical increase in the resistance arising from self-induction, the power of the machine will be affected—

1st. By the magnetic field due to the field-magnet being rotated, and therefore weakened by the rotation of the armature.

2nd. By the magnetic field due to the current in the armature being added to that due to the field-magnet.

3rd. By an actual back electro-motive force set up in the armature of a dynamo, for the reasons to be explained further on.

It has usually been assumed that the magnetic field produced by the current flowing round the armature must, both in a dynamo and in a motor, weaken the one set up by the field-magnet. It has further been assumed, that since a forward lead is given to the brushes in a dynamo on account of the delay in the magnetisation, that therefore a backward lead ought to be given to the brushes in a motor, to give time for the current to magnetise the iron of the armature. And lastly, it has been taken for granted, that because, in both a dynamo and in a motor, the motion of the armature and field-magnet relatively to one another produces an electro-motive force, therefore, since experience had shown that a powerful field-magnet and an armature of a comparatively weak magnetic moment were the best for producing a large electro-motive force in a dynamo, the same proportion of field-magnet and armature must be the best to produce a powerful motor. As these three assumptions appeared to us unproved, we have thought it desirable to investigate what ought to be the relative strength of the magnetic fields produced by the field-magnet and by the armature in a motor.

In considering the strength of the field produced between field-magnet and armature by the field-magnet and armature currents respectively, if F and A are certain functions of the currents and number of windings in the field-magnet and armature respectively, then F + A represents the total number of lines of force in the field, or what we may call the total strength of the field when the brushes have 90° of lead, and the armature and field-magnet are at rest. With any other lead, F will remain the same, but A will be reduced.

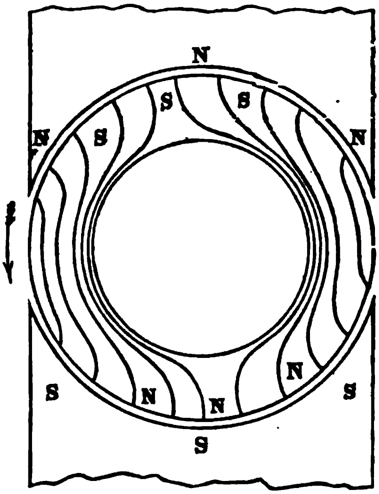


FIG. 15.

Both F and A may be expressed in the form $\frac{k\,C}{1 + s\,C}$.

If the armature and field-magnets are rotating relatively to one another, then the field due to the field-magnet will be twisted round, and consequently weakened.

Fig. 15 shows approximately, in the case of a Gramme machine, the distribution of the lines of force due to the field-magnet as twisted round by the rotation of the armature.

If, now, θ is the angular distance from the position of a brush when there is no lead, then the number of lines of force passing through a coil of the armature at any place due to the field-magnet alone is proportional to

$$f(n)\ \mathrm{F}\ \cos.\ (\theta - \gamma),$$

where γ is a function of n, the number of revolutions per minute. It seems, from the results of such experiments as have hitherto been made, that $f\ (n)$ and $-\gamma$ are nearly constant for small values of n, and diminish the more rapidly as n becomes considerable. The shapes of these functions vary with the shape of the armature and the nature of the iron. It is of course obvious that, instead of the simple periodic function cos. $(\theta - \gamma)$, we ought to employ a Fourier's series to express the condition of a particular machine, a very important term in this series having a periodicity defined by the number of coils on the armature if any iron protrudes between the coils. But for general purposes the simple form is sufficiently correct. Now, if the brushes have a lead, ϕ, in the direction of motion, it is obvious that the electro-motive force due to rotation will be proportional to

$$n\ \mathrm{W}'\ f(n)\ \mathrm{F}\ \cos.\ (\phi - \gamma),$$

where W' is the number of windings on the armature, since, as the distribution of lines of force at each brush is symmetrical, the electro-motive force due to rotation must be proportional to the number of lines of force cutting a ring at a brush—that is, must be proportional to the value of the last expression, when θ equals ϕ.

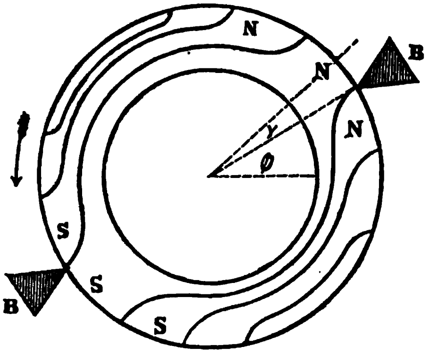


FIG. 16.

If the field produced by the current flowing round the armature were symmetrical, and not affected by the iron of the field-magnet, as it is often erroneously supposed to be, and if it were merely twisted round by rotation, as shown in Fig. 16, then the number of lines of force passing through a coil of the armature at any place due to the current in the armature alone would be proportional to

$$+\ f(n)\ \mathrm{A}\ \sin.\ (\theta - \phi - \gamma);$$

and, as before, the electro-motive force arising from the rotation of the coils in this field would be proportional to the value of this last expression, when θ equals ϕ, or to

$$-\, n\, W'\, f(n)\, A \sin.\, \gamma.$$

In the case of a dynamo machine, the resultant magnetic field is usually supposed to be due to the fields shown in Fig. 15 and in Fig. 16, for which latter case the sign of the last expression is, as stated, negative. In a motor, as we shall show further on, the field due to the armature may assist that due to the field-magnet, so that the distribution of lines of force due to the armature shown in Fig. 16 may be adopted, but with the N and S interchanged, and in that case the number of lines of force passing through a coil of the armature at any place due to the current in the armature alone would be proportional to

$$-\, f(n)\, A \sin.\, (\theta - \phi - \gamma),$$

and the electro-motive force arising from the rotation of the coils in this field would be proportional to

$$+\, n\, W'\, f(n)\, A \sin.\, \gamma.$$

Hence, if the field due to the armature were a symmetrical field, the poles being at the brushes when the armature is at rest and only slightly shifted round by the rotation, as shown in Figs. 15 and 16, then in a dynamo machine the electro-motive force would be proportional to

$$n\, f(n)\, W' \{F \cos.\, (\phi - \gamma) - A \sin.\, \gamma\},$$

and in a motor to

$$n\, f(n)\, W' \{F \cos.\, (\phi - \gamma) + A \sin.\, \gamma\}.$$

In Fig. 17, B B is the development of the centre line of the ring from brush to brush. The curve F shows by its ordinates the number of lines of force passing through the armature coils at every part of the ring, due to the field-magnet action only; the curve A shows the same thing in the case of the dynamo, and A′ in the case of the motor, for the armature magnetic action only. D shows the sum of F and A for a dynamo, and M the sum of F and A′ for a motor. The electro-motive force in the case of a dynamo will be proportional to 2 B D, and in a motor to 2 B M.

We have already stated that we do not recognise the assump-

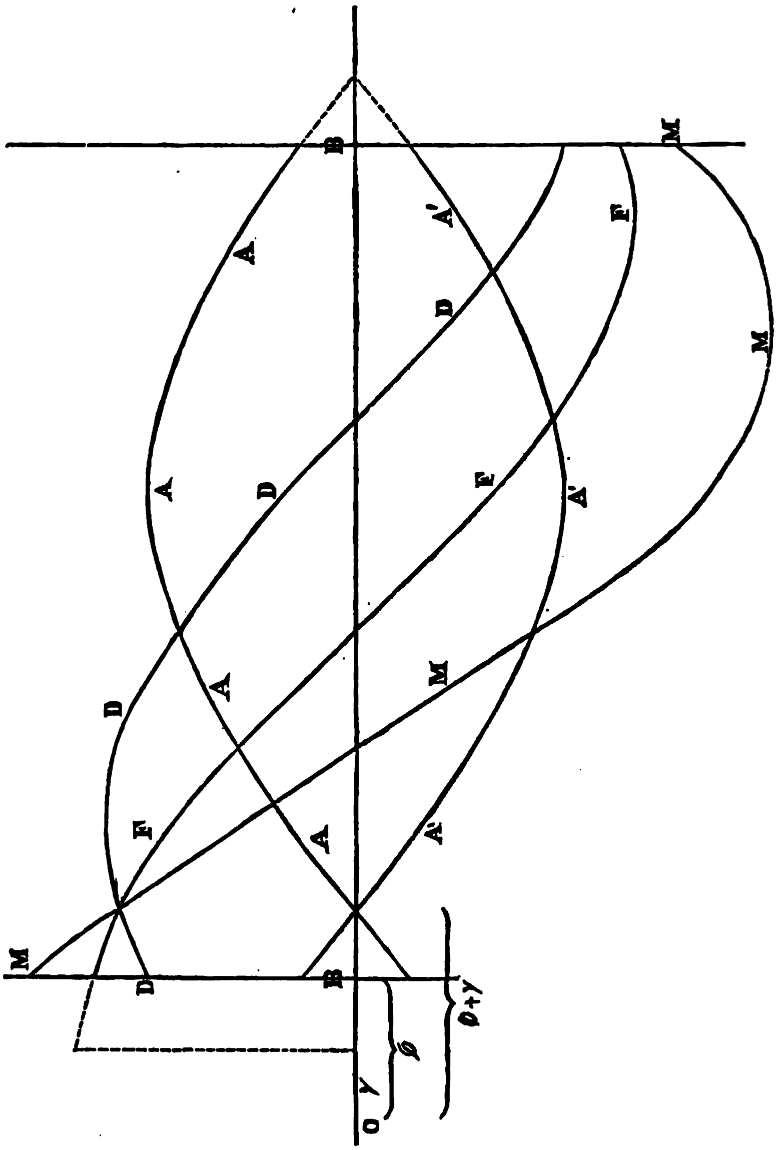


Fig. 17.

tion usually made that the armature field must weaken the field-magnet field in a motor as it does in a dynamo; but if the lead of the brushes in a motor is sufficiently far back, then the armature field will no doubt weaken the field-magnet field, as in the case of a dynamo, and the negative sign above must be taken.

To illustrate this, here is one of our motors so arranged that the wires passing round the field-magnet can be short-circuited, in which case the magnetism in the field-magnet will only be that produced by the induction of the armature. The lead of the brushes is put back so that the armature field weakens the field-magnet field as in an ordinary dynamo. Now observe what happens. On starting the current the motor works as an ordinary motor, but on merely short-circuiting the field-magnet coils it stops, and rotates nearly as forcibly in the opposite direction. When the current then passed through the field-magnet there were strong negative forces opposing the motion, but which were overcome by the current passing through the field-magnet coils. The resultant action, therefore, was a difference, and consequently small. Now let us turn the brushes so as to give a more forward lead; the two actions help one another; the resultant is a sum, and the motor works more vigorously.

Although the adding together of the magnetic fields in motors is thus produced by a forward lead of the brushes, it may be possible in some cases that, owing to the iron of the armature being very hard, γ is so large that a backward lead of the brushes in a motor may produce an addition of the magnetic fields corresponding with a positive sign in the expression for the electro-motive force. But under such circumstances $f(n)$ will be small, as rapid rotation will seriously diminish the magnetic moment produced by the current. In all cases, however, what has to be aimed at in a motor is the adding together of the magnetic fields produced by the field-magnet and by the armature.

We cannot lay too much stress on the importance of this fact,— a motor must work mainly by attractive forces, and not by repulsive forces. The magnetic fields due to the field-magnet and armature must help one another, and not oppose one another, as in the case of a dynamo.

DISTORTION PRODUCED BY THE IRON OF THE FIELD-MAGNETS OF THE FIELD DUE TO THE ARMATURE.

When an ordinary dynamo machine is used as a motor, the attractive forces due to induction of the armature on the poles of the field-magnet are small, but in these motors of ours they are large, since the magnetic moment due to the armature coiling is large; and we wish to show that in an efficient motor the attractive forces ought to be made large, whereas in a dynamo they ought to be small.

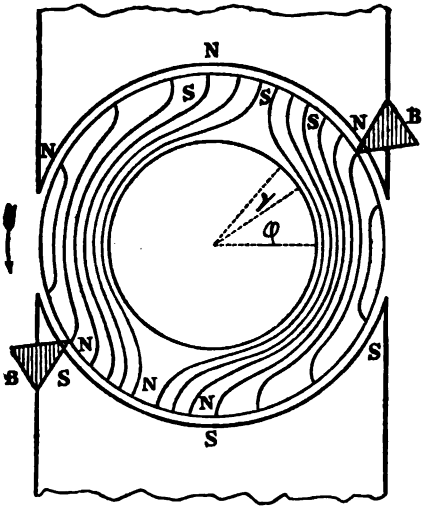


FIG. 16A.

In fact, in making any calculations from the distribution of lines of force, we must remember that not only does the iron in the armature determine the distribution of the lines of force due to the field-magnet, but that the iron in the field-magnet affects the distribution of the lines of force due to the armature. Indeed, as shown in Fig. 16A, corresponding with a motor with a forward lead, the field due to the armature is by no means the same in the four quarters of the ring. The polar extensions of the field-magnet cause the intense part of the field to be at N and S, instead of at the brushes B B; and not only is the field twisted round, but we have on one side of N or of S in the armature a more sudden diminution of the field strength than on the other side. In fact, instead of the curve of cosines, we have the curve shown in A, Fig. 18. Combining this with the field-magnet distribution given already, and represented in F, Fig. 18, we have the sum of these shown at M as the distribution for a motor, and the difference shown at D for a dynamo machine.

The electro-motive force in a dynamo is therefore proportional to 2 B D, and in a motor to 2 B M.

Generalising roughly, for all shapes of machines it may be said that, whilst the electro-motive force due to the rotation remains what it was before for the field-magnet distribution, viz., $n\ \mathrm{W}'\, f\,(n)\ \cos.\ (\phi - \gamma)$, it assumes a different form for the armature from that previously obtained.

In the previous case, when the action of the iron of the field-magnets on the armature field was neglected, it was easy to

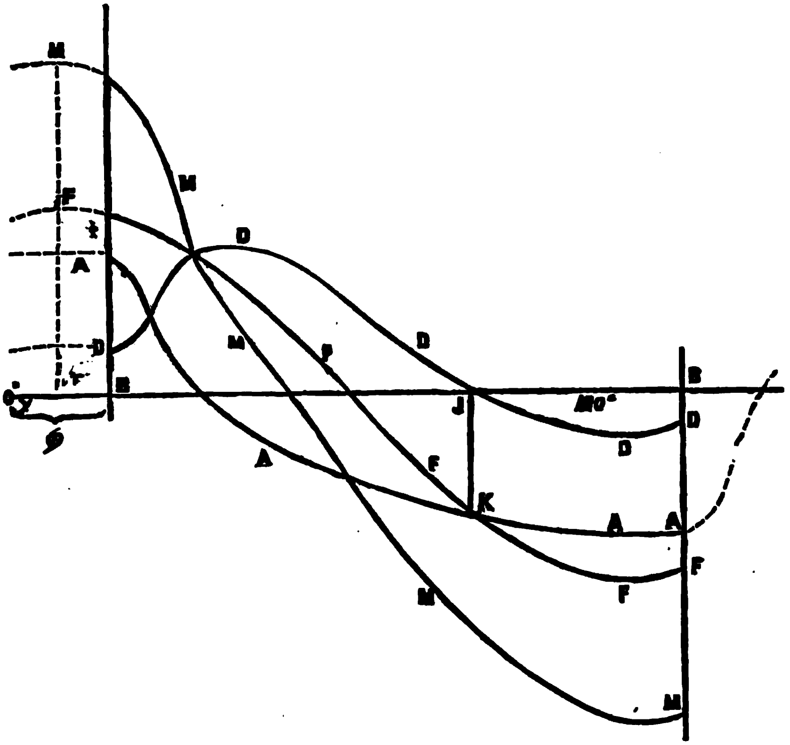


Fig. 18.

express the equation for the curve A A A, Fig. 17, in terms of θ, ϕ, and γ, and from this, by making θ equal to ϕ, to find the expression for the electro-motive force due to the armature field. But in the second case, when the disturbing action of the soft iron of the field-magnets is taken into account, we cannot from the one curve A A A, Fig. 18, express its equation in terms of θ, ϕ, and γ, seeing that the one curve alone gives no indication of how its

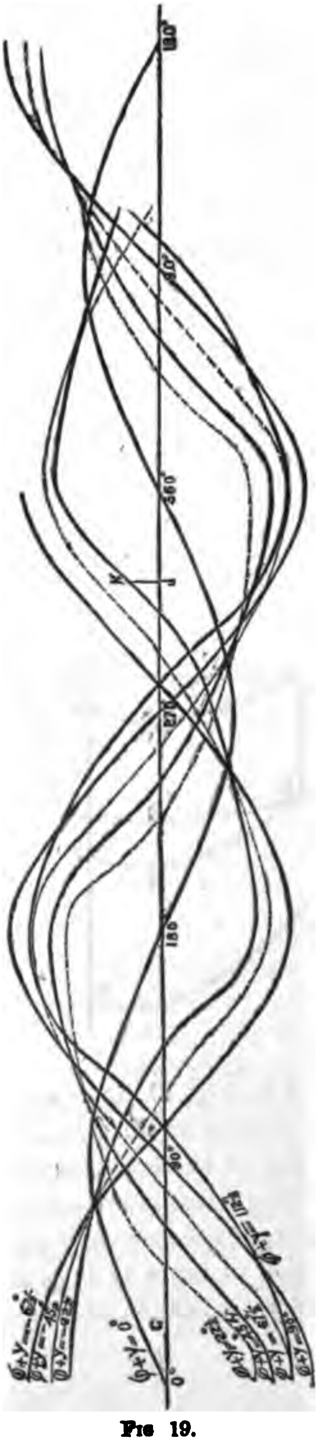


Fig 19.

form varies with changes in ϕ and γ. We have therefore in Fig. 19 drawn the curve A A A for various values of $\phi + \gamma$, equal respectively to $-67\frac{1}{2}°$, $-45°$, $-22\frac{1}{2}°$, 0°, $22\frac{1}{2}°$, 45°, $67\frac{1}{2}°$, 90°, $112\frac{1}{2}°$.

As before, the horizontal line represents the development of the centre line of the ring, and the ordinates of any particular curve the number of lines of force due to the armature field as affected by rotation and by the presence of the iron of the field-magnets which pass through a coil on the armature at any angular distance, θ, from the origin for that value of $\phi + \gamma$ for which the particular curve has been selected.

From these curves in Fig. 19, by giving to γ the values 0°, $22\frac{1}{2}°$, 45°, $67\frac{1}{2}°$, and 90°, we have calculated numbers proportional to the respective electro-motive forces due to the rotated and distorted armature field divided by the speed of rotation. This we have done by measuring the ordinate which in each case for each curve corresponds with the position of the brush. For instance, to find the $\frac{\text{E.M.F.}}{n}$ when $\phi + \gamma$ equals 45° and γ equals $22\frac{1}{2}°$, we must first select the curve corresponding with $\phi + \gamma$, equal to 45°; next we see that the brush which has the lead ϕ or $(\phi + \gamma) - \gamma$ is really at $22\frac{1}{2}°$ from the zero. Hence G H, to the left of the figure, is proportional to the $\frac{\text{E.M.F.}}{n}$ in this case. The following table gives all the results so obtained:—

$\phi + \gamma$.	E.M.F. $\div\, n$ for the following values of γ.				
	$\gamma = 0$.	$\gamma = 22\frac{1}{2}°$.	$\gamma = 45°$.	$\gamma = 67\frac{1}{2}°$.	$\gamma = 90°$.
$-67\frac{1}{2}$	$-0·62$	0·67	1·87	2·75	3·33
-45	$-1·00$	0·12	1·12	1·92	2·46
$-22\frac{1}{2}$	$-1·79$	0·12	0·87	1·57	2·00
0	0	0·72	1·33	1·83	2·00
$22\frac{1}{2}$	1·75	2·50	2·47	2·33	2·00
45	1·01	2·42	3·00	2·83	2·75
$67\frac{1}{2}$	0·58	1·88	3·04	3·50	3·29
90	0	1·46	2·75	3·62	4·00
$112\frac{1}{2}$	$-0·77$	0·67	1·83	2·67	3·22

From these numbers we have drawn curves shown in Fig. 20, connecting $\phi + \gamma$ and $\frac{\text{E.M.F.}}{n}$ for each of the values of γ; and, finally, from these latter curves we find that the electro-motive force due to the rotation of the coils in the armature field, as twisted by rotation and distorted by the iron of the field-magnets, is proportional to

$$(1 + 0·8 \cos. \gamma) \sin. 2\,(\phi + \tfrac{1}{2}\gamma) + 0·5 \cos. 2\,\gamma$$
$$\times \sin. 4\,(\phi + \tfrac{1}{2}\gamma) + 3 \sin. \gamma,$$

or, finally, that the total electro-motive force in a dynamo or motor due to all causes is

$$n f(n)\,\text{W}'\left[\text{F} \cos. (\phi - \gamma) \mp \frac{\text{A}}{4}\{(1 + 0·8 \cos. \gamma) \sin. 2\,(\phi + \tfrac{1}{2}\gamma)\right.$$
$$\left. + 0·5 \cos. 2\,\gamma \sin. 4\,(\phi + \tfrac{1}{2}\gamma) + 3 \sin. \gamma\}\right],$$

the negative sign being taken for a dynamo, and the positive sign for a motor. For the numbers 0·8 and 0·5 and 3 in this expression, literal coefficients may be applied to express the general condition.

Now as to the scale of these diagrams, it will be seen from what has gone before that ½ A is to be used to express the greatest number of lines of force passing through any part of the armature when ϕ equals 90° and γ equals 0°, and, according to our scale of measurement employed in the diagrams above, A equals 4.

This expression, although complicated in appearance, will be found very useful, since it takes into account all the disturbing action arising from the action of the iron of the rotating armature on the field due to the field-magnet, as well as the disturbing action of the iron of the field-magnet on the field set up by the current flowing round the rotating armature.

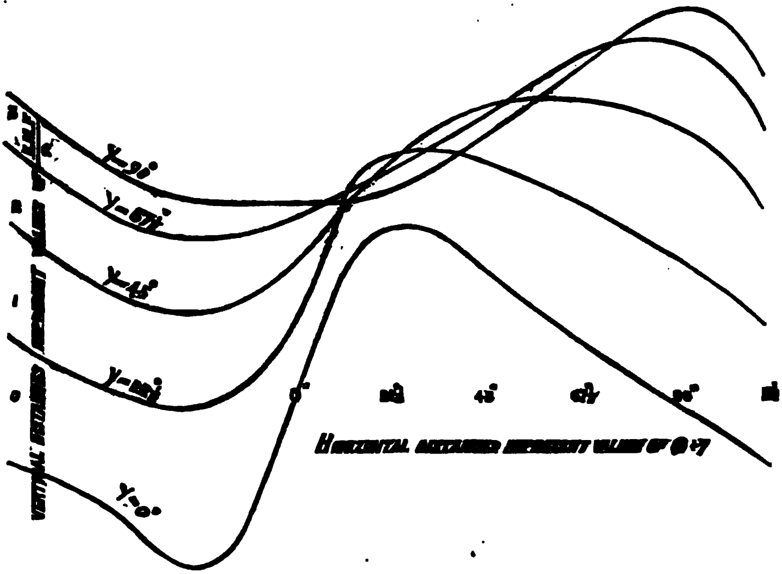


Fig. 20.

Best Relative Sizes of the Armature and Field-Magnet in a Dynamo and in a Motor.

Having arrived at the complete expression for the electromotive force in a dynamo or in a motor, we can now determine the proper relative proportions to give to the armature and field-magnet in each type of machine.

Let C and C′ be the currents passing round the field-magnet and armature respectively; then, since W and W′ are the numbers of convolutions on the field-magnet and armature,

$$F : A :: \frac{W\,C}{1 + s\,C} : \frac{W'\,C'}{1 + s'\,C'},$$

or if the iron cores of both the field-magnet and of the armature are far from saturation, then, approximately,

$$F : A :: W C : W' C'.$$

On referring back to the complete expression for the electro-motive force in terms of n, ϕ, and γ, we see that we may roughly state the second part as $W' A \sin. 2 (\phi + \frac{1}{2}\gamma)$. Hence for a rough generalisation we may say that the electro-motive force equals

$$n f(n) W' (F \cos. \phi \mp A \sin. 2 \phi),$$

and using the last proportion, the electro-motive force E is proportional to

$$n f(n) W' (W C \cos. \phi \mp W' C' \sin. 2 \phi),$$

or the electrical power of the dynamo or motor

$$C' E \propto C' n f(n) W' (W C \cos. \phi \mp W' C' \sin. 2 \phi).$$

Now let us assume that the field-magnet and armature form together one continuous magnetic circuit, such as would be produced by an anchor ring all wound with coils of wire forming one or more distinct circuits, and traversed by currents all flowing in the same direction; then, if all parts of this magnetic circuit be equally heated, the magnetic moment of any part will be simply proportional to its length, whatever be the number of convolutions on this particular part or the currents flowing through them. Hence, if the field-magnet and armature in a dynamo or motor be equally heated by the currents passing round them, W C and W′ C′ will be simply proportional to the lengths of the two portions into which we divide this closed magnetic circuit, and which are called respectively field-magnet and armature. If, therefore, L and l be the lengths of the magnetic circuit given up to the field-magnet and to the armature respectively, $L + l =$ constant, say, K, for a machine of a given type and weight. And since, as we have shown,

$$C' E \propto n f(n) (L l \cos. \phi \mp l^2 \sin. 2 \phi),$$

we have to determine L, l, and ϕ, so that the latter expression may become as large as possible.

First, let us take the case of the negative sign, and let ϕ be positive (that is, the machine is a dynamo with a forward lead or a lead in the direction of rotation), then it will be found that the expression has a maximum for a very small value of l. For example, if ϕ is 30°, then the maximum is obtained when l equals $\frac{K}{4}$. If ϕ is 45°, then l equal to $\frac{7}{34}$ K makes the expression a maximum, etc. In other words, in a dynamo machine in which the brushes have, as is usually the case, a forward lead, the armature should be very small compared with the field-magnet, and should be the smaller the greater is the lead of the brushes.

Second, let us take the case of the positive sign, and let ϕ be positive, which corresponds with the case of a motor having a forward lead, then it will be found that, except for very small values of ϕ, the expression for the electro-motive force has no maximum in the mathematical sense of the expression, but, instead, that as l increases the electro-motive force continuously increases; hence, for any value of ϕ, such as 20°, l should be made as large as possible, to make the electro-motive force as large as possible. In a motor, then, the armature should be by far the largest part of the machine.

If we assume merely that the weight of the machine is a constant, which is roughly the same thing as saying that the sum of the volumes of the field-magnet and armature is a constant, and if we assume that in making the field-magnet or armature larger or smaller in volume we increase the dimensions in all directions, so that the shorter part is thick and the long part thin, then we shall have some such relation as $A^2 + F^2 =$ a constant, say, K^2, if the volumes of the two component parts of the machine are considered as being proportional to the squares of their greatest magnetic strengths.

If, now, we take the case of a motor, that is, use the positive sign in the expression for the electro-motive force, then we find that a maximum is obtained when

$$\begin{aligned} F^2 &= \tfrac{1}{10} K^2, \\ A^2 &= \tfrac{9}{10} K^2, \\ \text{and sin. } \phi &= \tfrac{2}{3}, \\ \text{or } \phi &= 42°, \text{ approximately.} \end{aligned}$$

Hence the armature must have 90 per cent. of the whole weight. and the field-magnets only 10 per cent.

On the other hand, if for the dynamo we take the same value of ϕ with the negative sign, then we find that the maximum electro-motive force is obtained when it is the armature that has only about 10 per cent. of the whole weight, and the field-magnet 90.

We have every reason to believe, therefore, that an investigation concerning these relative sizes in a motor made to any design such as we can at present imagine, if soft iron is used in the field-magnet, must have the result that the armature part ought to form a much greater part of the whole weight than the field-magnet part, whereas in a dynamo machine it is the armature to which the very small weight is given.

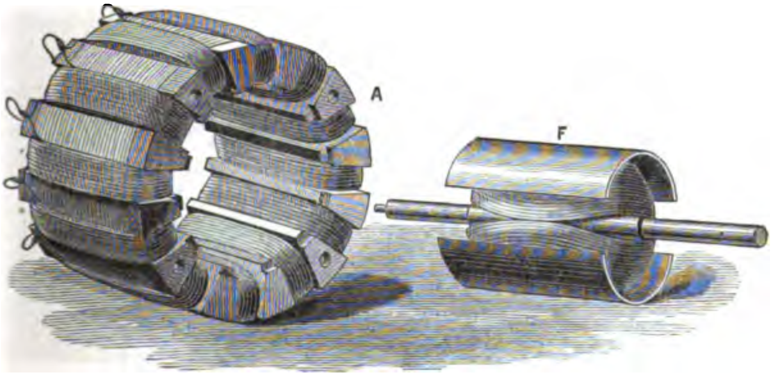


Fig. 21.

When we consider, however, that the apparent resistance of a motor or dynamo due to self-induction is in proportion to the armature strength, we find that it is necessary to make the armature less in importance than might be assumed from the above investigation, and our own practical rule at present in the motors shown to the meeting consists in making the field-magnet about one quarter of the weight of the armature.

It must be remembered that it is not merely a condition for greatest steady power of a motor or dynamo which we have been looking for, but also a condition for greatest efficiency with a greatest steady power, because of two machines of the same power, when heated up to less than a destructive temperature, the larger will waste most power.

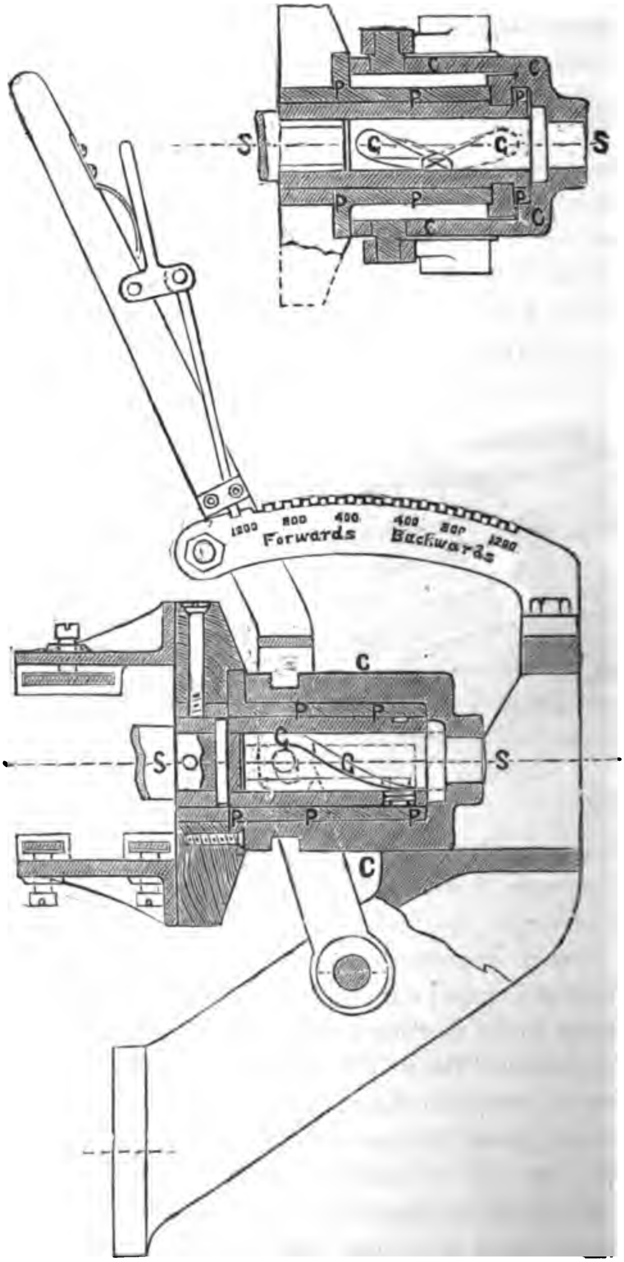

C
C
P
P
P
S
C
C
S
P
P
P
C
C
1200
800
400
400
800
1200
Forwards
Backwards
C
P
P
S
C
C
S
P
P
P
C

Fig. 22.

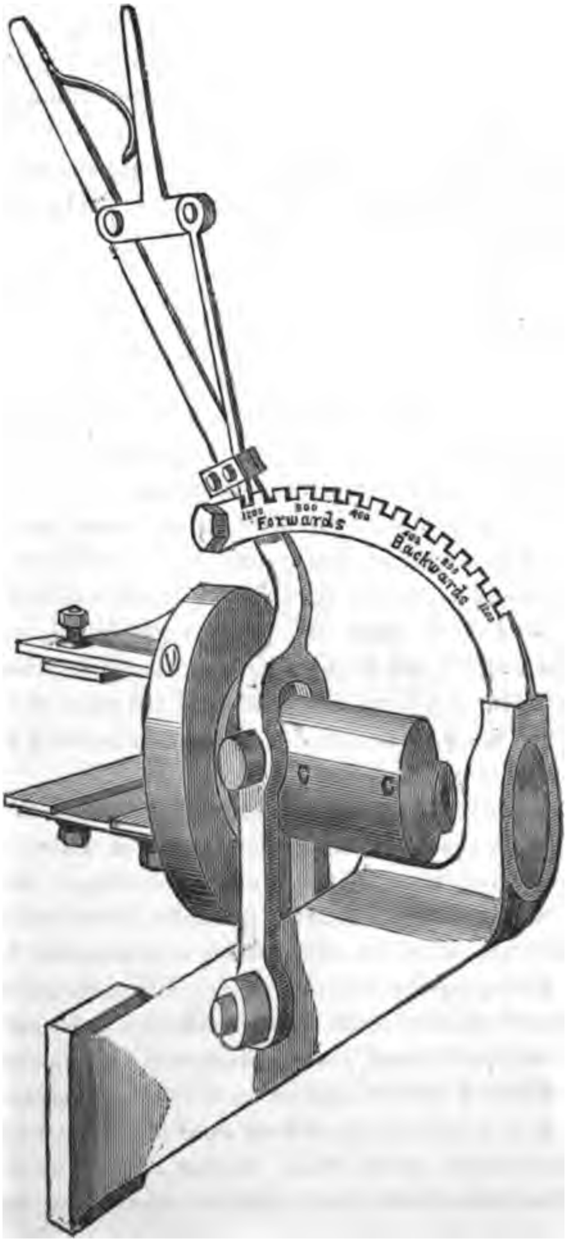


FIG. 23.

Now an armature, from its shape, is necessarily a weak magnet, while the field-magnet, from its shape, is a strong one; hence in our motors we have reversed the usual condition of things, and made the armature large and the field-magnet small, and from this we have been led, as will be seen from the accompanying figure (No. 21), to make the armature stationary and surrounding the field-magnet, instead of, as is usual, the field-magnet surrounding the armature.

Our small field-magnet, F, then carries the brushes and revolves inside the stationary armature, A, the coils of which, as is seen in Figs. 1, 2, 3, and 24, are joined to the stationary commutator, C C, which on some of our motors we make flat, to save both space and expense, while in others it is cylindrical, as this gives rise to less friction than the flat commutator. Wherever the brushes, B, happen to be at any particular moment, their two opposite magnetic poles are produced on the armature, or rather the poles are produced at two points in the armature, a little behind the brushes. As the brushes run round and round, so do these poles, and the brushes, which, be it remembered, are carried by the field-magnet, are so set that the magnetic poles in the armature are always a little way in front of the poles of the field-magnet. The latter, therefore, are perpetually running after the former, but never catching them.

We have spoken about the lead of the brushes, which we have seen is of great practical importance, since the amount of lead decides not merely which way the motor revolves, or whether it revolves at all, but also whether the fields due to the field-magnet and armature are added to one another or subtracted from one another in producing the resultant field. Although the fact that the direction of rotation could be reversed by a sufficient change in the lead was well known, the importance of varying the lead in motors for different speeds appears to be little attended to, since, as a rule, it is impossible in motors, from their construction, to make small changes in the lead. In our motors, on the other hand, the flat brush-holder can be revolved forwards or backwards by hand through any angle, so that any lead forwards or backwards can be given to the brushes for each speed, and in our larger motors, one of which is shown in Fig. 3, the lead can be made anything we like simply by moving a handle, H, such as a locomotive engine-driver is accustomed to use for acting on the link-motion of his engine. Push the handle, shown enlarged in Figs. 22 and 23, forwards, and the motor revolves rapidly in one direction; pull it backwards, and it revolves as rapidly in the other direction. Push it not so far forwards, and it does not revolve so fast in the first direction; pull it not so far backwards, and it does not revolve so fast backwards. In fact, by moving the handle forwards or backwards, any speed in either direction is obtainable. The action of our "lead adjuster" is as follows:—

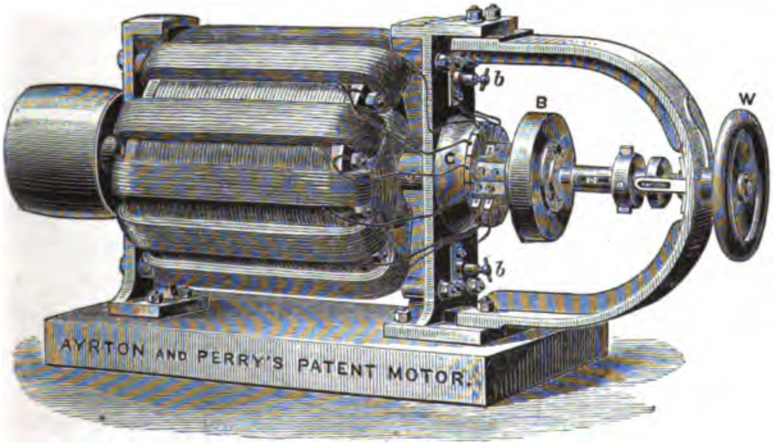

b
B
W
C
b
AYRTON AND PERRY'S PATENT MOTOR.

Fig. 24.

Attached to the rotating field-magnet is the spindle, S S, which is itself attached to, and rotates with, the outer collar, C C. On pushing the handle forwards or backwards, this collar is moved along the spindle, and the effect of this is to cause a pin to move along the groove, G G, and so cause the inner collar. P P, which usually rotates along with C C and the field-magnet, to move a little forwards or backwards relatively to C C. Now it is the collar, P P, which is screwed to the brush-holder, hence we are able, even when the motor is running, to shift the brushes relatively to the field-magnet together with which they are rotating, and consequently to give any lead forwards or backwards we desire. In other cases we alter the lead by means of a wheel, W, and screw, as seen in Fig. 24, and so get very easily a very accurate adjustment. A table of speeds can be engraved, showing the position the handle should be in for 500 revolutions a minute, or 700 revolutions a minute, etc.

EFFECT OF LEAD ON THE BRUSHES ON SPARKING.

We have already explained that one cause for the sparking at the brushes arises from self-induction, but there will be another cause arising from an electro-motive force existing in the coil short-circuited at a brush, if such a coil be cutting lines of force owing to the brush not being at a neutral part of the resultant field. Hence the importance of placing the brushes at the neutral part of the resultant field. In a dynamo a forward lead will do this, as shown from the following figure (No. 25), where S N represents the magnetic axis of the field in the armature, produced by the current flowing round the field-magnet, and modified by all the iron in the machine; N′ S′ the magnetic axis of the field produced in the armature by the current flowing round the armature, and modified by all the iron in the machine; and *s n* the axis of

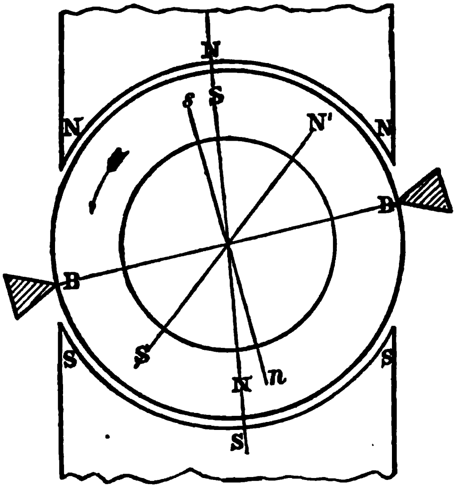


FIG. 25.

the resultant magnetic field, which is at right angles to B B, the line of the brushes.

If the machine is a motor with a forward lead, then the polarity of N′ S′, due to the armature coils, must be reversed, and we have the arrangement shown in Fig. 26. The axis of the resultant fields is now not perpendicular to the line of the brushes, and so there must be some sparking; but the two are more perpendicular than in Fig. 27, where the direction of rotation is reversed, and which would correspond with a dynamo having a backward lead. Consequently, while in a dynamo a back lead of the brushes, corresponding with the two fields strengthening one another, would be to some extent balanced by excessive sparking, this is not so much the case in a motor with a forward lead of the brushes, since the twist in the field, due to the rotation, assists the perpendicularity of magnetic axes in a motor while it diminishes it in a dynamo. And as a proof that putting the brushes with a backward lead in a motor, so as to prevent all sparking, is really detrimental to the power and efficiency of the motor, we append the following results of experiments made by us on this subject, using a motor weighing thirty pounds, consisting of a Gramme ring rotating inside a Siemens field-magnet:—

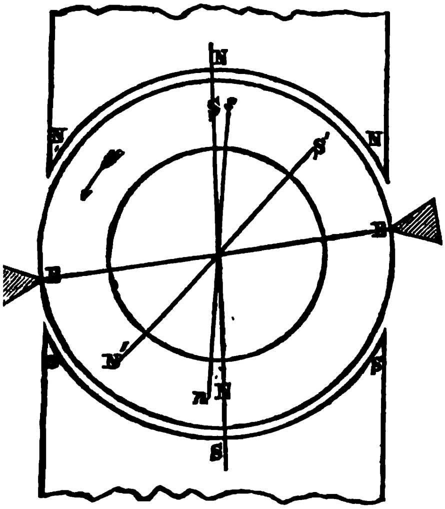


FIG. 26.

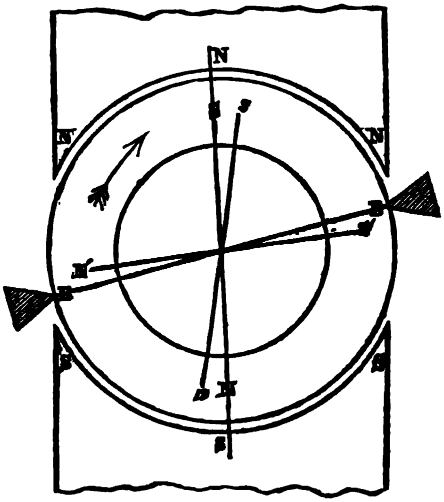


FIG. 27.

Lead.	Remarks.	Revolutions per minute.	Current in ampères.	Potential difference at terminals in volts.	Load on dynamometer in lbs.	Horse-power given in.	Horse-power given out.	Efficiency per cent.
− 45°	Very little sparking at the brushes ...	3,162	11·2	35·6	1·1	0·535	0·116	21·6
0°	Much sparking at the brushes	3,045	9	41·2	,,	0·499	0·111	22·3
− 20°	Less sparking at the brushes	3,130	10·1	42·5	,,	0·576	0·115	19·9
0°	Much sparking at the brushes	3,248	8·2	42·7	,,	0·468	0·119	25·4
− 20°	Less sparking at the brushes	3,405	9·4	42·7	,,	0·538	0·124	23·1
0°	{Sparking partly due to the brushes requiring trimming ... ... }	3,147	9·0	42·9	,,	0·519	0·115	22·2
	As the brushes were observed to be not making as good contact as possible with the commutator they were here taken off and trimmed, and the experiments repeated.							
− 30°	Little sparking at the brushes	3,228	9·7	42·5	,,	0·578	0·118	20·4
0°	Much more sparking at the brushes ...	3,181	8·2	42·5	,,	0·469	0·116	24·8

A negative lead means a lead in the direction of the lead ordinarily given to the brushes in a motor, or against the direction of rotation.

From an examination of the potential difference at the terminals with the current sent through the motor in each case, it will be seen that for a lead of 0° there is the highest back electromotive force. Further, from an examination of the table of efficiencies it is seen that a lead of 0° gives a higher efficiency than a negative lead.

It has already been pointed out that the condition of non-sparking at the brushes, apart from self-induction, is the same as the condition that the number of lines of force cutting a coil should not alter as the coil passes a brush; and, referring to Fig. 18, we see that this is the same as saying that the brush should be at the top of a crest or at the bottom of a wave in the curve for the resultant number of lines of force cutting a coil. In the case of the curve D, corresponding with a dynamo with a forward lead, we see that this condition is fulfilled with considerable accuracy, and also that it is approximately fulfilled in the case of the curve M, corresponding with a motor with a forward lead.

In Fig. 27 we have already seen roughly what would happen if we employed a backward lead in a dynamo. Let us now, with the aid of the curves shown in Fig. 19, consider how far the employment of the sum of the strengths of the fields due respectively to the armature and field-magnet, instead of the difference, which is all that is ordinarily made use of, would be beneficial. Let us take $\phi + \gamma$ equal to $-22\frac{1}{2}°$, and ϕ to about $-35°$; that is to say, let there be a back lead of 35°. The number of lines of force then cutting a coil at a brush, due to the armature field as affected by the rotation, as well as by the distorting action of the field-magnet, will be represented by something like J K in Fig. 19, corresponding with an angular distance of $-35°$ in the curve corresponding with $\phi + \gamma$ equal to $22\frac{1}{2}°$. Now J K is very small, and, further, the number of lines of force due to the field-magnet cutting the coil when at a brush at $-35°$, corresponding with J K, Fig. 18, is also much smaller than its value, B K, at the brush when it has a forward lead. Hence the sum of these two sets of lines of force for a back lead may not be so large, compared with the corresponding differences when the brush has a forward lead, as to counterbalance the injurious effect of sparking.

DETERMINATION OF THE POWER AND EFFICIENCY OF MOTOR. ABSORPTION DYNAMOMETERS.

The power in watts given to a motor, or indeed to any circuit to which electric energy is being supplied, is equal to the current, C, in ampères passing through the circuit multiplied into the potential difference, V, in volts at the terminals. A portion, $C^2 R$, of the power so furnished to the motor is wasted in heating the motor, due partly to the statical resistance of the motor, R_0, or resistance of the motor at rest, and partly to the increase in the resistance, $n\,p$, already referred to, and which is produced by self-induction. The remainder of C V, which is equal to $C\,v$ where v is the back electro-motive force in volts produced by the motion, is converted immediately into mechanical energy of rotation, but not necessarily into useful energy, since a portion of the power the motor produces is spent in overcoming the mechanical friction at the bearings and the mechanical friction of the brushes rubbing on the commutator, while another portion is spent in overcoming air friction and what may be called magnetic friction—that is, the resistance arising from the generation of Foucault currents in all pieces of metal, the number of lines of force cutting which is alternately increased and diminished as the movable part of the motor rotates; and it is only a third portion of the power the motor develops which is usefully given out, this latter, in watts, being equal to the torque, T, multiplied by n, the number of revolutions per minute, where T therefore stands for the moment of the twisting couple exerted by the moment in pound feet divided by 44·25.

The following table shows graphically the way in which the power supplied to a motor is used up:—

$$C\,V\left\{\begin{array}{l} C^2\,R\left\{\begin{array}{l} 1.\ C^2\,R_0. \\ 2.\ C^2\,n\,p. \end{array}\right. \\ C\,v\left\{\begin{array}{l} 3.\ \text{Power spent in overcoming the friction at the} \\ \quad\ \text{bearings and the friction of the brushes} \\ \quad\ \text{against the commutator.} \\ 4.\ \text{Power spent in overcoming magnetic friction} \\ \quad\ \text{arising from the creation of Foucault cur-} \\ \quad\ \text{rents and in overcoming the friction of the} \\ \quad\ \text{air.} \\ 5.\ \text{Power usefully given out by the motor, and} \\ \quad\ \text{equal to } T\,n. \end{array}\right. \end{array}\right.$$

The real power of the motor is simply of course T n, and the efficiency only $\frac{T\,n}{C\,V}$. In theoretical calculations not unfrequently 4 is totally neglected; indeed, in the published calculation of the efficiency of a well-known motor that recently was brought to our notice, 2, 3, and 4 were neglected, and the efficiency was said to be 90 per cent. Indeed, $C^2\,n\,p$ is almost invariably neglected, so that only $C^2\,R_0$ is assumed to be the waste of energy from heating, whereas it must necessarily be $C^2\,(R_0 + n\,p)$, even if all masses of metal in which Foucault currents might be generated were so electrically subdivided as to diminish the generation of such currents to a very small amount.

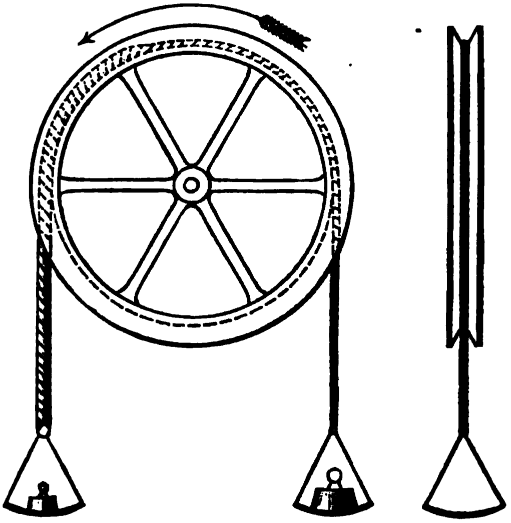
To measure, then, the power and efficiency of a motor, the important things to observe are C, V, T, and n. T may be measured with some form of absorption dynamometer, of which we have found the compensating forms devised by Professor James Thomson, by M. Carpentier, and by M. Raffard very convenient, while the older Prony brake can be made to give fairly good results for a short time if a constant stream of water be allowed to flow over it.

In making experiments on motors, it is very important to be able to keep the torque constant while changes are made in the speed, in order, for example, to see exactly how for a constant torque the current varies with the speed. With the simplest form of absorption dynamometer, consisting of a cord or band passing over a pulley, and having unequal weights attached to its extremities, this is almost impossible, since, in order to keep the difference of the weights constant, it is necessary, if the coefficient of friction between the cord and the pulley alters, to alter the smaller weight, and hence also the larger. This is easily seen from the equation

$$P - Q = Q\left(2{\cdot}71828^{k\frac{l}{r}} - 1\right)$$

where P and Q are the two weights, k the coefficient of friction, l the length of the cord wrapped on the pulley, of which the radius is r. Now, as even with careful lubrication it is difficult to keep k, the coefficient of friction, perfectly constant, and as also it is difficult to arrange to increase or diminish both weights as the coefficient alters, it follows that this simple dynamometer cannot be employed, as the cord is constantly tending to slip off on one side or the other of the pulley. If a spring balance be attached to the cord on the side on which the tension is greatest, the arrangement becomes automatically adjusting, but it does so by P varying to suit k, hence $P - Q$ is not a constant. The Prony brake is to a certain extent an improvement on this, because alterations in the coefficient of friction can to a certain extent be compensated for by tightening up the screws of the brake-block by hand, without altering the weights attached to the end of the lever. The compensating dynamometers referred to above are, however, better still, since with these the length of band wrapped round the pulley is automatically adjusted as the coefficient of friction alters. And further, the torque, which is proportional to P – Q can be increased simply by increasing P, and without altering Q, since here, again, l automatically changes to suit this change.

In spite, however, of these advantages, there are certain disadvantages attending the use of such a compensating dynamometer, arising from the fact that it is quite a distinct instrument from the motor, and has to be carefully coupled to the shaft of the motor, considerable precaution being necessary, even when a flexible coupling is employed, to avoid friction being introduced by a strain on the two shafts.



AYRTON AND PERRY'S COMPENSATING DYNAMOMETER.
FIG. 28.

We have therefore set ourselves to devise some simpler form of compensating absorption dynamometer, and have arrived at excellent results by using a most simple expedient. In the Prony brake the amount of pressure between the brake and the pulley is adjusted by screwing up the tightening screws by hand, to compensate for changes in the coefficient of friction: in the Thomson, Carpentier, and Raffard dynamometer it is the length of the band wrapped round the pulley that is automatically altered; but the plan we adopt consists in keeping the length of the cord fixed, but automatically varying the nature of the cord in contact with the pulley. Figure 28 shows the exact arrangement we employ. On the shaft of any motor of which the power and efficiency are to be tested, is keyed a grooved wheel of suitable diameter, and in the grove of this wheel is placed a cord carrying two unequal weights at its ends in the ordinary way. The two halves of the cord are of very unequal thicknesses and coefficients of friction, the junction between the two portions being slightly tapered. The thicker and rougher part of the cord carries the smaller weight, and is therefore on the descending part of the wheel, and the compensating action, which we find to be very perfect, it is very easy to understand. If from the drying up of the lubricant, or from any other cause, the coefficient of friction between the grooved wheel and the cord becomes greater, the larger weight is raised, and were there no adjusting arrangement it would be thrown over the wheel. But the ascending of this larger weight causes less of the rougher, and more of the smoother cord to be wrapped on the wheel. Hence the larger weight rises only far enough for this change to be effected to a sufficient degree to compensate for the increase in the coefficient of friction. And if the coefficient of friction diminishes, it is the larger weight which descends a certain amount, causing more of the rougher, and less of the smoother cord to be wrapped on. It is obvious that, by using a grooved wheel of sufficiently large diameter, the sum of the weights, and consequently the pressure of the motor shaft on its bearings, corresponding with any given torque, can be made as small as we like.

It was not until near the completion of this investigation in motors that we devised the new form of compensating absorption just described; hence the majority of our experiments have been made with a Raffard dynamometer. We have already explained the importance of using a flexible coupling between the shaft of this dynamometer and the motor shaft; and in the case of small motors, where the useful torque is necessarily small, the utmost care must be taken to avoid the introduction of even a small unknown resistance at this coupling, if the maximum power of the motor is to be obtained. With such small motors, we have found that a long spiral spring, with coils of comparatively small diameter, formed a very good flexible coupling.

And the same kind of spiral spring coupling we have used to connect the speed counter, or the speed indicator, with the motor, since the ordinary plan of pressing by means of the hand the speed counter or indicator against the revolving spindle of the motor is quite inadmissible when testing small motors, as the variable hand-pressure introduces a serious unknown error into the results. A speed indicator which gives the speed at any moment is the more convenient, but its calibration must not be forgotten to be made, by comparing its readings at different speeds with the results obtained with a speed counter, seeing that the majority of the speed indicators we have met with have had quite appreciable errors in the graduation. We have found it, however, quite easy to obtain very consistent readings by using a speed counter turning constantly with the motor; for although the counter is continually revolving, we find it possible, at a speed of 2,000 revolutions a minute, not to make an error of more than 6 or 7 revolutions, or only about $\frac{3}{10}$ per cent., by observing it when in motion at the beginning and end of a certain interval of time.

CONNECTION BETWEEN BACK ELECTRO-MOTIVE FORCE AND SPEED OF A MOTOR.

Considering the somewhat complicated way in which the fields due respectively to the armature and to the field-magnet are distorted, each by the iron core of the other, and that both fields are carried round through a certain angle by the motion, it is very striking to find even in our motors, where not merely the field-magnet field but also the armature-field is made intentionally important, that, whatever be the lead of the brushes, the back electro-motive force is proportional to the speed if the currents passing through the motor be kept constant. Thus, for example, with our normal size motor here on the table, with six positions of the brushes differing successively by fifteen degrees, we obtain as the result of a large number of experiments,

$$\begin{aligned} e &= n \times 0{\cdot}01193 \text{ for } \phi = 9^\circ \\ e &= n \times 0{\cdot}01160 \text{ ,, } \phi = -6^\circ \\ e &= n \times 0{\cdot}01110 \text{ ,, } \phi = -21^\circ \\ e &= n \times 0{\cdot}01050 \text{ ,, } \phi = -36^\circ \\ e &= n \times 0{\cdot}00901 \text{ ,, } \phi = -51^\circ \\ e &= n \times 0{\cdot}00589 \text{ ,, } \phi = -66^\circ \end{aligned}$$

where n is the number of revolutions per minute.

The e here mentioned is obtained by subtracting $C R_0$ from the potential difference established at the motor terminals. To obtain the true back electro-motive force, v, we ought, as already explained, to subtract a term, $C n p$, in consequence of the increase of the resistance arising from the rotation. Hence, for a constant current, if e is, as we find, proportional to the speed, so ought v to be. It follows, then, that although γ increases with the speed, and although the expression given on page 333 for the true back electro-motive force increases with γ, still it is a constant for a constant value of C and ϕ. Now, for a constant current, v may be expressed as a function of n multiplied by a function of ϕ and n, or

$$f(n) \times f^1 (\phi_1 n);$$

and v we have proved is independent of n if ϕ is constant. Hence when n is altered, $f(n)$ must vary in inverse proportion to $f^1(\phi_1 n)$.

If we consider the above-mentioned values of e, and divide them by n, we obtain a series of constants for each lead; or, if we plot the values of e for a fixed current and for a fixed speed, we obtain the form of $f^1 (\phi_1 n)$, since v and e for a fixed current and a fixed speed only differ by a constant. This we have done in Fig. 29 for a speed of 2,000 revolutions per minute, in which case the following were the corresponding values of e and ϕ:—

e.	ϕ.
23·85	+ 9°
23·2	− 6
22·21	− 21
21·01	− 36
18·02	− 56
11·78	− 66

It is interesting here to notice the fact on which we have already laid great stress, namely, that a back lead, although it may cause less sparking, does so at the expense of the back electro-motive force, which we observe diminishes steadily as the lead becomes more and more negative.

We have already seen that

$$v = n\,\chi\,(\phi, \mathrm{C}),$$

and
$$\mathrm{C\,V} = \mathrm{C}^2\,(\mathrm{R}_0 + n\,p) + \mathrm{C}\,v;$$

also that
$$\mathrm{C}\,v = \mathrm{P} + n\,r + \psi,$$

where P is the power utilised, $n\,r$ the power wasted in material

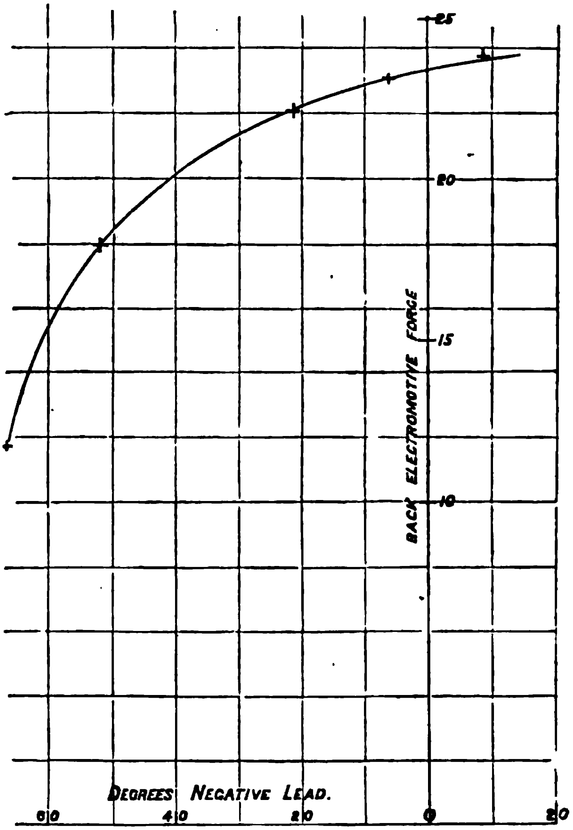


FIG. 29.

friction, and ψ the power wasted in magnetic friction and friction with the air. Hence the efficiency of the motor E

$$= \frac{n\,\mathrm{C}\,\chi\,(\phi, \mathrm{C}) - n\,r - \psi}{\mathrm{C}^2\,(\mathrm{R}_0 + n\,p) + n\,\mathrm{C}\,\chi\,(\phi, \mathrm{C})},$$

so that if ϕ and C are constants

$$E = \frac{A - \frac{\psi}{n}}{\frac{B}{n} + D}.$$

But we find that, whatever the lead of the brushes may be, there is a certain speed for every lead beyond which not only does the efficiency diminish, but it diminishes proportionately to the increase of speed—that is, as soon as $\frac{B}{n}$ becomes insignificant in comparison with D, we have E following a law of the shape

$$E = a - b\,n.$$

Hence we see that ψ must be proportional to the square of n —that is, the waste of power due to magnetic friction and the friction of the air must be proportional to the square of the speed, or the frictional force is proportional to the speed, the ordinary rule for fluid friction.

The following sets of numbers obtained from experiments with one of our normal size motors, weighing 36 lbs., before making the improvements described further on, are examples of the results from which we have deduced the law just stated :—

PERCENTAGE EFFICIENCY AT THE FOLLOWING SPEEDS.

LEAD.	REVOLUTIONS PER MINUTE.							
	600	800	1,000	1,200	1,400	1,600	1,800	2,000
$\phi = -66°$	25·8	29·0	29·9	30·3	29·4	28·1	26·5	24·6
$\phi = -51°$	26·2	28·6	29·5	28·6	26·6	23·4	21·4	18·0
$\phi = -36°$	24·9	26·0	26·3	25·6	24·2	22·0	18·9	14·8
$\phi = -21°$	26·2	27·1	26·0	23·6	20·8	16·2	11·6	6·0
$\phi = -6°$	22·6	25·4	24·0	21·2	17·0	12·0	5·8	—
$\phi = 9°$	20·3	20·6	19·8	17·9	14·0	8·0	0·9	—
$\phi = 24°$	17·4	17·4	16·6	14·7	12·0	6·9	2·6	—
$\phi = 39°$	12·9	13·4	10·2	8·9	4·2	—	—	—

The two great sources of loss of power in dynamos and motors are—

1. Loss by electric resistance.
2. Loss by magnetic friction.

In small motors we find that when the first of these is great the second is small, and when the second is great the first is small. This is to be expected; for when the magnetic field for a given current is made very strong, by means of pieces of iron protruding from the armature, the motor becomes a powerful one, only a small amount of power being wasted in heat due to wire resistance. But this very protrusion of pieces of iron produces necessarily discontinuity in the magnetic field, so that, although the average field set up by the armature through which the field-magnet moves is a constant relatively to the field-magnet, it undergoes fluctuations, and so causes heat in the iron of the field-magnet, in addition to the heat set up in the iron of the armature by the constant reversals of the current in it. Thus we find in our motors, where the resultant magnetic field due to the armature and field magnets is very powerful, that it is magnetic friction that we have to strive to diminish; whereas in motors made on the Gramme or Siemens principle it is the heating due to resistance that is so much in excess. Similarly in the De Meritens machine, a complete set of experiments on which is about to be given, the magnetic friction is comparatively small.

This being the case, we use these observations to illustrate how any machine may be taken up by a student, and the law for its efficiency and power determined without his binding himself down to any theory of the loss due to magnetic friction. But when taken up in this way, even the experiments on the De Meritens machine show the loss due to magnetic friction is proportional to the square of the speed.

This object alone would not have been a sufficient reason for putting before the members such a large number of results of experiments, but we give these numbers mainly because we are not aware that any such complete set of observations, so carefully made on a motor, has yet been published. And as there are many persons who are anxious to investigate this important question of motors, and who have no opportunity of making observations for themselves, it has appeared to us desirable that accurate data should be at their disposal.

EXPERIMENTS ON DE MERITENS' MOTOR, MONDAY, 26TH FEBRUARY, 1883, AND THE FOLLOWING DAYS.

Speed.	Current in ampères.	Potential difference at the motor terminals in volts.	W_1	W_2	$W_1 - W_2$	Horse-power put in.	Horse-power taken out.	Efficiency E	Resistance R_0	$\frac{V}{n}$	e	$\frac{e}{n}$	REMARKS.
1,504	8·4	28·1	1·6	4	2·4	·318	·162	·51	...	·0186	...	...	W_1, W_2, and $W_1 - W_2$ are in kilogrammes. Lead of the brushes the same throughout all the experiments.
1,796	7·5	30·4	2·4	4	1·6	·304	·129	·42	...	·0169	...	...	
1,482	9·8	29·8	5	8	3	·392	·192	·49	...	·0208	...	...	
1,868	10·0	29·8	6	9	3	·399	·184	·46	...	·0217	...	...	
1,260	11·1	28·6	7·4	11	3·6	·428	·203	·47	...	·0226	...	...	
1,290	10·2	28·6	7	10	3	·390	·173	·44	...	·0221	...	...	
1,552	10·7	32·5	7	10	3	·464	·208	·45	...	·0209	...	...	
1,302	12·2	31·5	8	12	4	·516	·233	·45	...	·0241	...	...	
1,188	13·9	32·9	10	15	5	·614	·266	·43	...	·0267	...	...	
1,208	13·9	32·2	10	15	5	·602	·271	·45	...	·0266	...	...	
1,152	14·6	31·2	10	16	6	·611	·310	·51	...	·0270	...	...	
1,048	15·3	30·7	10	17	7	·632	·329	·52	...	·0292	...	...	
1,008	16·4	31·0	12	19	7	·681	·316	·46	...	·0307	...	...	
1,030	15·0	30·5	14·2	20	5·8	·613	·268	·44	...	·0296	...	...	
572	13·1	19·8	13	18	5	·349	·128	·37	...	·0363	...	...	
612	12·3	19·8	14	18·2	4·2	·327	·115	·35	...	·0323	...	...	
706	13·1	22·9	14	19	5	·408	·158	·39	...	·0324	...	...	

EXPERIMENTS ON DE MERITENS' MOTOR—*continued.*

Speed.	Current in ampères.	Potential difference at the motor terminals in volts.	W_1	W_2	$W_1 - W_2$	Horse-power put in.	Horse-power taken out.	Efficiency E	Resistance R_0	$\frac{V}{n}$	e	$\frac{e}{n}$	REMARKS.
932	13·9	26·4	14	19	5	·490	·209	·42	...	·0283	...	...	
2,266	12·7	47·1	10	14	4	·802	·406	·51	...	·0208	...	...	
2,066	14·4	46·5	11	16	5	·899	·463	·51	...	·0210	...	...	
1,986	14·1	45·3	11	16	5	·855	·445	·52	...	·0228	...	...	
2,006	12·3	42·2	12	16	4	·695	·359	·52	...	·0210	...	...	
1,932	12·7	41·5	14	18	4	·707	·346	·49	...	·0214	...	...	
1,085	10·2	23·8	10	13	3	·327	·146	·45	...	·0219	...	...	
1,844	15·6	44·0	13	18	5	·916	·413	·45	...	·0238	30·9	0168	To take this speed used counter.
1,850	15·1	45·6	13	18	5	·91	·4	·44	...	·0248	32·9	·0177	Now used the indicator to take the speed with.
1,940	14·7	44·9	13	18	5	·883	·435	·49	...	·0231	32·5	·0167	
1,820	14·7	44·7	13	18	5	·87	·39	·46	...	·0246	32·4	·0178	
1,930	14·3	43·0	13	18	5	·81	·42	·53	...	·0228	31·0	·0160	
2,400	14·7	61·8	13	18	5	1·19	·52	·44	...	·0257	49·4	·0206	
2,947	15·6	61·5	13	18	5	1·25	·64	·52	...	·0208	48·4	·0164	
2,904	15·6	60·9	13	18	5	1·21	·63	·52	...	·0209	47·8	·0164	
1,660	10·1	32·5	11	14	3	·42	·21	·51	...	·0195	24·0	·0145	
1,600	10·6	32·5	11	14	3	·44	·20	·47	...	·0203	23·6	·0147	

1,840	24·4	55·7	20	30	10	1·77	·804	·45	...	·0302	35·2	·0192	
1,920	23·3	55·4	20	30	10	1·68	·84	·50	...	·0288	35·8	·0187	
1,520	23·0	47·0	20	30	10	1·41	·664	·47	...	·0309	27·7	·0182	
1,520	22·9	47·0	20	30	10	1·4	·664	·47	...	·0309	27·7	·0182	
1,090	23·2	38·9	20	30	10	1·17	·477	·41	...	·0357	19·4	·0178	
1,120	22·6	39·2	20	30	10	1·15	·49	·43	...	·0352	10·2	·0180	
600	22·5	30·7	20	30	10	·895	·262	·29	...	·0514	11·8	·0201	
580	22·5	30·4	20	30	10	·887	·254	·29	...	·0524	11·5	·0193	
335	22·4	24·1	20	30	10	·695	·146	·21	...	·0719	5·3	·0154	
330	22·5	24·4	20	30	10	·706	·145	·20	...	·0739	5·5	·0169	
320	22·5	24·4	20	30	10	·706	·14	·20	...	·0765	5·5	·0171	Took these with the counter.
2,412	24·2	67·3	20	30	10	2·12	1·05	·50	...	·0279	46·9	·0194	
2,936	24·6	65·8	20	30	10	2·11	1·28	·61	...	·0258	45·1	·0154	
2,032	24·2	63·8	20	30	10	2·01	·89	·44	...	·0313	43·5	·0213	
2,700	25·4	71·6	20	30	10	2·38	1·18	·50	...	·0262	50·3	·0186	
1,000	7·6	20·3	3	5	2	·198	·087	·44	...	·0208	13·9	·0139	
1,000	7·5	20·0	3	5	2	·193	·087	·45	...	·0200	13·7	·0137	
720	7·6	16·0	3	5	2	·154	·063	·41	...	·022	9·6	·0134	
440	7·7	12·2	3	5	2	·118	·038	·33	...	·0277	5·7	·013	
940	7·4	18·0	3	5	2	·169	·082	·49	...	·0191	11·8	·0125	
1,200	7·8	22·3	3	5	2	·223	·105	·47	...	·0185	15·7	·0131	
1,450	8·0	26·1	3	5	2	·27	·127	·47	...	·018	19·4	·0133	
1,650	8·2	29·6	3	5	2	·313	·144	·46	...	·0179	22·7	·0137	

EXPERIMENTS ON DE MERITENS' MOTOR—*continued.*

Speed.	Current in ampères.	Potential difference at the motor terminals in volts.	W_1	W_2	$W_1 - W_2$	Horse-power put in.	Horse-power taken out.	Efficiency. E	Resistance R_0	$\frac{V}{n}$	e	$\frac{e}{n}$	REMARKS.
1,850	8·3	33·1	3	5	2	·358	·162	·45	...	·0178	26·1	·0141	
2,200	8·6	36·8	3	5	2	·425	·197	·46	...	·0167	29·6	·0134	
1,800	8·8	33·1	3	5	2	·361	·157	·44	...	·0183	26·1	·0145	
2,142	8·4	36·0	3	5	2	·393	·187	·47	...	·0168	28·9	·0135	
2,460	8·6	40·0	3	5	2	·447	·215	·48	...	·0162	32·8	·0133	
2,656	8·8	44·1	3	5	2	·507	·231	·46	...	·0188	36·7	·0138	
2,820	9·0	47·0	3	5	2	·551	·246	·45	...	·0166	39·4	·0139	
3,200	9·0	50·4	3	5	2	·592	·288	·47	...	·0157	42·8	·0133	
3,400	9·3	52·8	4	4	2	·641	·297	·46	...	·0158	45·1	·0132	
3,500	9·8	56·2	4	4	2	·686	·306	·45	...	·0160	48·4	·0138	
480	11·0	15·7	6	10	4	·230	·086	·39	...	·0327	6·6	·013	
484	11·4	15·7	6	10	4	·239	·087	·37	...	·0324	6·1	·0127	
795	11·5	21·4	6	10	4	·330	·142	·44	...	·0269	11·7	·0148	
620	10·8	20·3	6	10	4	·294	·111	·39	...	·0327	11·2	·0181	
700	11·1	20·0	6	10	4	·297	·125	·43	...	·0285	10·7	·0153	
724	10·8	20·0	6	10	4	·290	·129	·45	...	·0276	10·9	·0151	
1,000	10·8	23·5	6	10	4	·341	·179	·54	...	·0235	14·4	·0144	

1,000	10·8	23·5	6	10	4	·341	·179	·54	...	·0235	14·4	·0144	
860	10·6	21·4	6	10	4	·306	·154	·51	...	·0248	12·5	·0145	
1,320	11·4	29·3	6	10	4	·447	·237	·53	...	·0222	19·7	·0149	
...	32·0	27·3	...	...	...	...	...	...	·852	...	...	...	
1,165	10·8	27·3	6	10	4	·395	·209	·53	...	·0234	18·2	·0156	
1,400	11·4	31·0	6	10	4	·474	·250	·53	...	·0221	21·4	·0152	
...	33·2	29·0	...	...	...	...	...	...	·874	...	...	...	
1,560	11·4	32·8	6	10	4	·500	·280	·56	...	·021	24·5	·0154	
1,575	12·3	34·8	8	12	4	·574	·282	·57	...	·0221	26·2	·0155	
1,690	11·6	35·9	8	12	4	·561	·303	·55	...	·0212	28·3	·0167	
...	15·2	12·2	...	...	...	...	...	...	...	...	...	...	
1,780	11·9	38·3	8	12	4	·610	·319	·52	...	·0215	28·5	·016	
1,870	12·1	37·0	8	12	4	·660	·335	·51	...	·0192	26·8	·0143	
1,964	12·5	41·8	8	12	4	·697	·352	·50	...	·0218	31·8	·0159	
2,064	12·5	43·8	6	12	4	·731	·37	·51	...	·0212	33·3	·0161	
2,260	12·3	45·2	8	12	4	·746	·405	·54	...	·02	34·9	·0154	
...	18·8	15·1	...	...	...	...	...	...	·803	...	...	...	
2,356	12·3	47·3	8	12	4	·779	·422	·54	...	·02	37	·0157	
2,428	12·5	49·3	8	12	4	·823	·435	·53	...	·0208	38·8	·0159	
2,536	12·5	50·5	8	12	4	·848	·454	·54	...	·0199	40	·0157	
2,716	12·5	52·8	8	12	4	·881	·487	·55	...	·0195	42·3	·0155	
...	19·3	15·1	...	...	...	...	...	...	·782	...	...	...	
2,804	12·7	55·4	8	12	4	·943	·502	·53	...	·0197	44·6	·0159	

EXPERIMENTS ON DE MERITENS' MOTOR—*continued.*

Speed.	Current in ampères.	Potential difference at the motor terminals in volts.	W_1	W	$W_1 - W_2$	Horse-power put in.	Horse-power taken out.	Efficiency E	Resistance R_0	$\frac{V}{n}$		$\frac{}{n}$	REMARKS.
2,720	13·1	54·5	11	15	4	·958	·487	·51	...	·02	43 5	·0159	
2,852	13·1	56 3	11	15	4	·989	·511	·52	...	·0197	45·3	·015	
3,010	13·1	58 3	11	15	4	1·02	·539	·53	...	·0193	47·4	·015	
...	12 9	9·6	...	...	...	...	...	...	·783	...	...	...	
930	15·2	28·4	12	18	6	·586	·250	·43	...	·0305	15·6	·0168	
710	14·8	24·4	12	18	6	·484	·191	·39	...	·0343	12·0	·0169	
620	14·7	22·6	12	18	6	·444	·167	·37	...	·0364	12·3	·0199	
1,120	15·6	32·2	12	18	6	·672	·3	·45	...	·0287	13·1	·0188	
1,280	16·0	35·4	12	18	6	·758	·344	·45	...	·0276	13·4	·0170	
1,480	16·0	38·9	12	18	6	·883	·398	·48	...	·0263	13·4	·0104	
1,660	16 2	43·5	12	18	6	·946	·489	·52	...	·0274	13·6	·018	
1,900	16·0	47·0	12	18	6	1·012	·51	·50	...	·025	13·5	·0176	
2,112	16·0	49·9	12	18	6	1·069	·567	·53	...	·0236	13·4	·0172	
2,488	16·3	55·7	12	18	6	1·217	·667	·55	...	·0223	13·7	·0168	
2,532	16·4	58·6	12	18	6	1·3	·68	·52	...	·0231	44·8	·0177	
...	20·6	17·1	...	...	...	...	...	...	·831	...	...	...	

...	16·7	14·5	...	...	...	...	...	...	·85	...	...	...	
510	18·4	23·8	15	23	8	·6	·183	·305	...	·0466	8·4	·0164	
700	18·5	27·5	15	23	8	·698	·251	·359	...	·0392	12·5	·013	
940	18·2	31·3	15	23	8	·77	·337	·43	...	·0322	16	·017	
1,120	18·9	34·8	15	23	8	·894	·402	·45	...	·0311	18·9	·0169	
1,300	18·9	38·3	15	23	8	·981	·466	·47	...	·0294	22·4	·0172	
1,400	19·8	42·1	15	23	8	1·129	·502	·45	...	·03	25·5	·0182	
...	24·4	19·7	...	...	...	...	...	...	·83	...	...	...	
1,600	20·1	46·1	15	23	8	1·257	·574	·46	...	·0288	29·2	·0182	
1,520	20·1	46·1	15	23	8	1·257	·545	·44	...	·0309	29·2	·0192	
1,700	19·7	45·8	15	23	8	1·22	·61	·5	...	·0269	29·4	·017	
1,670	19·7	47·3	15	23	8	1·26	·598	·47	...	·0283	30·8	·0184	
1,840	19·5	49·3	15	23	8	1·3	·66	·51	...	·0267	32·9	·0179	
...	22·2	18·3	...	...	...	...	...	...	·818	...	...	...	
1,820	19·3	49·3	15	23	8	1·29	·653	·50	...	·0276	33·1	·0181	
1,950	19·3	52·8	15	23	8	1·38	·7	·51	...	·027	36·6	·0187	
2,040	19·7	55·1	15	23	8	1·47	·73	·47	...	·0275	38·5	·0189	
2,200	19·8	56·8	17	25	8	1·53	·79	·52	...	·0257	40·2	·0182	
...	20·5	16·8	...	...	...	...	...	...	·834	...	...	...	
2,240	20·9	58·6	18	26	8	1·66	·8	·46	...	·0262	41·0	·0183	
2,540	20·5	62·3	18	26	8	1·73	·9	·53	...	·0245	45·3	·017	
...	20·5	16·5	...	...	...	...	...	...	·83	...	...	...	
2,200	19·8	58·0	18	26	8	1·55	·79	·51	...	·0263	41·4	·0188	

EXPERIMENTS ON DE MERITENS' MOTOR—*continued*.

Speed.	Current in ampères.	Potential difference at the motor terminals in volts.	W_1	W_2	$W_1 - W_2$	Horse-power put in.	Horse-power taken out.	Efficiency E	Resistance R_0	$\frac{V}{n}$	e	$\frac{e}{n}$	REMARKS.
2,640	19·8	62·6	18	26	8	1·674	·95	·56	...	·0248	46·0	·0174	
2,600	19·8	63·2	18	26	8	1·69	·93	·55	...	·0243	46·6	·0179	
...	20·5	17·1	...	...	...	...	...	...	·83	...	...	...	
600	5·2	9·9	2	3	1	·07	·026	·37	...	·0166	5·5	·0092	
...	12·3	10·1	...	...	...	...	...	...	·872	...	...	...	
280	5·0	6·1	2	3	1	·045	·012	·274	...	·0216	1·9	·0068	
620	5·0	9·9	2	3	1	·069	·027	·39	...	·0159	5·7	·0092	
1,000	5·0	13·9	2	3	1	·097	·044	·45	...	·0139	9·7	·0097	
1,010	5·0	13·9	2	3	1	·097	·045	·46	...	·0137	9·7	·0096	
...	16·4	18·0	...	...	...	...	...	...	·831	...	...	...	
1,160	5·7	17·4	3	4	1	·138	·052	·39	...	·015	12·6	·0108	
1,240	5·7	17·4	3	4	1	·138	·056	·41	...	·0131	12·6	·0101	
1,650	5·4	20·9	3	4	1	·151	·074	·49	...	·0128	16·4	·0099	
1,440	6·1	20·9	3	4	1	·172	·064	·37	...	·0145	15·8	·0109	
1,640	5·4	20·6	3	4	1	·149	·073	·49	...	·0125	16·1	·0098	
...	23·9	20·3	...	...	...	...	...	...	·848	...	...	...	
1,800	5·7	25·5	3	4	1	·196	·08	·41	...	·0141	20·7	·0115	

1,900	5·6	24·6	3	4	1	·18	·087	·47	...	·0129	19·9	·0105	Indicator.
1,900	5·6	24·6	3	4	1	·18	·087	·47	...	·0129	19·9	·0105	Counter.
2,20	5·8	28·4	3	4	1	·228	·098	·44	...	·0129	23·5	·0107	
...	30·3	25·8	...	...	...	...	...	...	·851	...	...	...	
2,280	5·7	28·7	3	4	1	·220	·102	·46	...	·0125	23·9	·0105	
2,500	6·1	31·9	3	4	1	·263	·112	·43	...	·0127	26·7	·0106	
2,600	5·7	31·9	3	4	1	·245	·116	·47	...	·0122	27·1	·0104	
2,700	5·8	35·1	3	4	1	·274	·121	·44	...	.013	30·2	·0112	
2,720	5·9	35·4	3	4	1	·278	·122	·44	...	·013	30·4	·0112	
...	20·5	16·2	...	...	...	...	...	...	·821	...	...	...	
3,080	5·9	40·0	3	4	1	·318	·138	·44	...	·0129	35·0	·0114	
...	...	...	...	...	...	...	...	...	Mean ·841	...	...	...	
5,200	4·6	40·0	No	load.		·246	...	...	...	·0076	36·1	·0069	

Horse-power taken out = speed × $(W_1 - W_2) \times$ ·0000448. The diameter of the pulley was 2½ inches, and the diameter of the pulley + the thickness of the belt was 2⅝ inches, giving a mean 2·5625 inches from which the above was calculated, W_1 and W_2 being in kilogrammes. The weight of the motor was 72 lbs.

The first thing we proceed to find is the dependence of torque on current. It is usually assumed that for a given current the torque is independent of the speed; but in making such an assumption the error is made of neglecting magnetic friction, and of assuming that the attraction between the field-magnet and armature is constant for a given current passing round them, the effect of the rotation, in first shifting the position of the poles, and, secondly, in weakening the strength of the resultant magnetic field due to the field-magnet and armature, being neglected. We, on the other hand, have invariably found that as the speed increases it requires a greater and greater current to produce a given torque.

The following gives a set of results obtained when using a motor weighing 30·8 lbs., consisting of a Gramme armature turning between a Siemens' field-magnet, the load on the dynamometer, or difference of weights, being kept quite constant at 0·88 lbs., and which corresponded with a useful power developed of 0·1 horse-power, at about 3,400 revolutions per minute:—

Number of revolutions per minute.	Current in ampères.
932	5·4
1,543	6·3
2,527	6·7
3,134	7·3
3,252	7·7
4,117	8·7

Fig. 30 shows the curves connecting currents with speeds for various constant torques in the De Meritens motor. The increase of current with speed is not nearly so great as in the case of the previous motor, but it is quite observable, and the curves are very interesting, as they enable us to calculate for the particular motor the current necessary to produce any particular torque at any speed. Thus, taking these measurements for speeds of 200 and of 3,000 revolutions per minute, we obtain the two sets of points shown in Fig. 31, evidently lying in the curves A B and C D. If T_{200} is the torque produced by a given current, C, when the speed is 200, and T_{3000} the torque produced by the same current when the speed is 3,000 revolutions per minute, we have found that $T_{200} - T_{3000}$ divided by C is nearly constant for all values of C

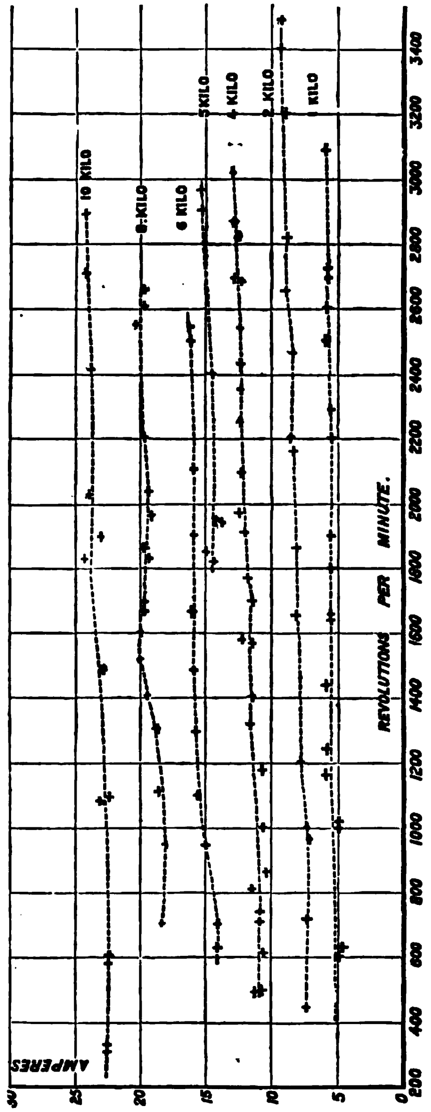


FIG. 30.

employed, and that the difference also seems to be proportional to the difference of speeds when other speeds are taken. Hence we arrive at the conclusion that in proper units the diminution in torque for a given current is

$$\cdot00000096\,n\,C.$$

Drawing, now, from this, the imaginary curve for no speed, and shown in E B, Fig. 31, we find that this curve satisfies the rational law,

$$T_0 = \frac{0\cdot002\,C^2}{1 + 0\cdot08\,C},$$

T_0 being the torque for a speed 0; so that for any other speed, n, we have

$$T = \frac{0\cdot002\,C^2}{1 + 0\cdot08\,C} - 0\cdot00000096\,n\,C.$$

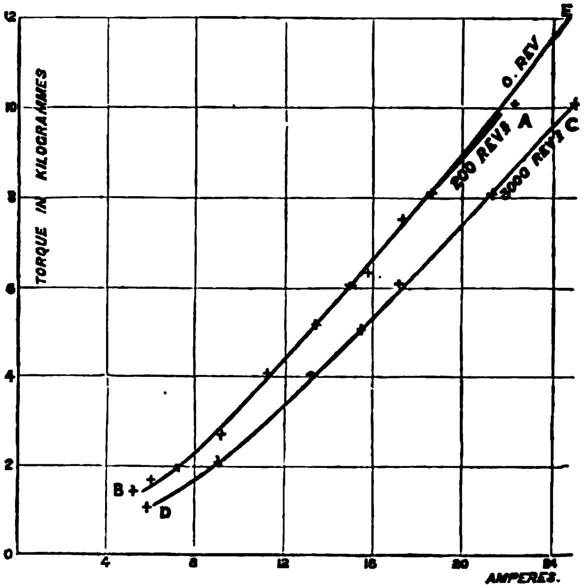


FIG. 31.

It will be observed that in these experiments the friction of the bearings of the motor and dynamometer is neglected, both being small.

Instead of using the above numbers, we shall say

$$T = C\,f(C) - a\,n\,C,$$

so that

$$\frac{T}{C} = f(C) - a\,n.$$

In Fig. 32 we have plotted $f(C)$, which is the value of

$\frac{T}{C}$, for n equals nought. We have also plotted $\frac{T}{C}$ for a speed of 3,000 revolutions per minute, and for a speed 0. The efficiency of the motor is, as already stated, $\frac{T\,n}{C\,V}$ or $\frac{T}{C} \div \frac{V}{n}$. In Fig. 32

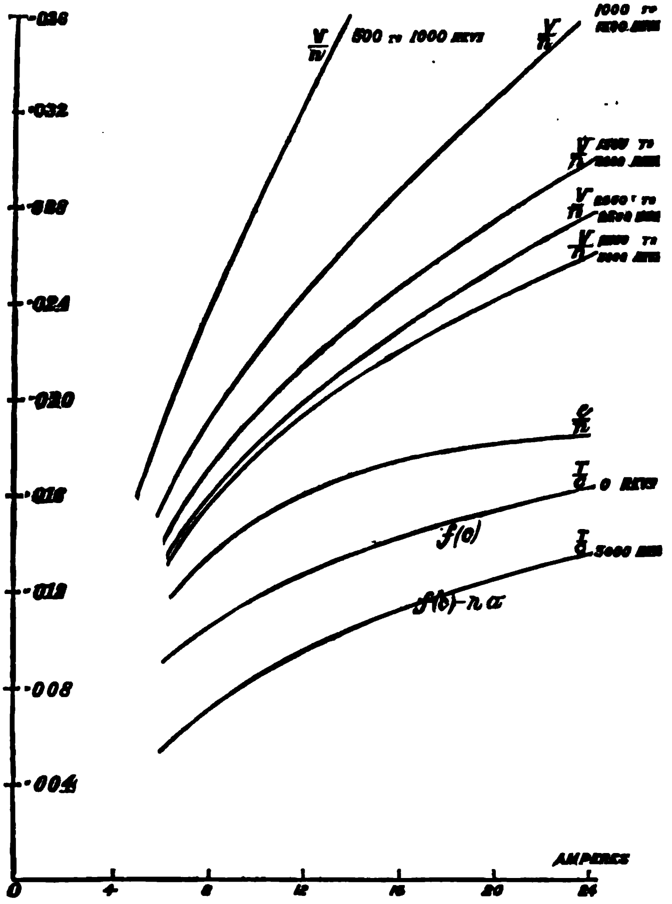


FIG. 32.

there are drawn the curves for $\frac{V}{n}$, corresponding with various speeds. Now, if we imagine a set of curves drawn for $\frac{T}{C}$, for the various speeds (all of which, for speeds between 0 and 3,000 revolutions a minute, will of course be between those drawn for

$\frac{T}{C}$ for n equal 0, and for n equal 3,000 respectively), then if two corresponding curves for $\frac{T}{C}$ and for $\frac{V}{n}$ be drawn for the same speed, the ordinate of the first curve divided by the larger ordinate of the second curve for the same current will give the efficiency of the motor for that speed and current. An examination of the curves shows that for great speeds there does not seem to be much variation in the efficiency, if we vary the speed while keeping the current constant, whereas at lower speeds the efficiency is much diminished by further diminishing the speed.

We next desire to plot a curve showing how the apparent back electro-motive force, e, or $V - C R_0$, divided by n, varies with the current in the De Meritens machine, but we find that this function is not independent of speed, as we have found it to be in our own motor. We have therefore assumed that the law is true, and taken an average curve for all our observations. Such a curve we find satisfies the law,

$$\frac{e}{n} = f(C) + \frac{C}{6000} + \frac{0{\cdot}0147}{C}.$$

Now

$$\frac{V}{n} = \frac{e}{n} + \frac{C R_0}{n},$$

and we have already proved that

$$\frac{T}{C} = f(C) - a\,n,$$

therefore the efficiency

$$E = \frac{C f(C) - a\,n\,C}{C f(C) + \frac{C^2}{6000} + 0{\cdot}0147 + \frac{C^2 R_0}{n}}$$

where

$$f(C) = \frac{0{\cdot}002\,C}{1 + 0{\cdot}08\,C}$$

and

$$a = 0{\cdot}00000096,$$

and the power in watts usefully given out by this motor

$$P = n\left\{C f(C) - a\,n\,C\right\}.$$

Hence we can at once calculate both the efficiency of this motor and the power it will usefully develop when running at any speed and when any current is flowing through it.

For the purpose of ascertaining what efficiency and power can practically be obtained with motors of different makers, of various sizes, running at different speeds, and with different loads, we have made a considerable number of experiments from time to time.

The smallest motor we have tested is a Griscom motor, weighing 2½ lbs. A great many experiments were made, running the motor at different speeds with different loads, but the highest load or difference of weights that could be used with a dynamometer, of which the circumference was 20 centimètres, without the current dangerously heating the motor, was found to be 0·55 lb., and a sample of the results so obtained is appended:—

Griscom Motor, 2½ lbs.

Revolutions per minute.	Current in ampères.	Potential difference at terminals in volts.	Load on dynamometer in lbs.	Horse-power given in.	Horse-power given out.	Efficiency.
633	6·3	10·6	0·55	0·0891	0·00693	0·0778
1,136	6·5	12·8	...	0·112	0·0124	0·111
1,691	6·8	14·9	...	0·135	0·0185	0·137
2,120	6·7	16·4	...	0·147	0·0232	0·158
2,433	6·7	17·4	...	0·156	0·0266	0·171

Resistance of this motor when warm was 1·1 ohms.

It was found, however, at the end of these experiments, that a current of 6·7 ampères was really far too much for this motor, seeing that, had we continued the experiments, the motor would have been probably damaged. We therefore ascertained what was the greatest current that could be sent for any considerable time through this motor without injuring it, and we found that a current of about 5 to 5½ ampères was the greatest. The horse-power given out by the motor in the preceding table is therefore higher than can actually be obtained in practice, and the results contained in the following set of experiments more nearly express the normal working conditions of the Griscom motor:—

Griscom Motor, 2½ lbs. (Second Set.)

Revolutions per minute.	Current in ampères.	Potential difference at terminals in volts.	Load on dynamo-meter in lbs.	Horse-power given in.	Horse-power given out.	Efficiency.
513	3·9	7·9	0·22	·0415	·00225	·0542
974	4·5	9·5	...	·0571	·00427	·0747
1,439	4·6	10·7	...	·0663	·00630	·0951
1,996	4·7	12·3	...	·0774	·00874	·113
2,520	4·9	13·3	...	·0874	·0110	·126

A useful power of about ·015 horse, with an efficiency of about 13 per cent., is the greatest that can be obtained for any length of time with the normal size Griscom motor.

The next motor in size employed was a motor with a Gramme armature and Siemens' field-magnet, weighing, with base plates, 8·03 lbs. Tests were made with loads varying from 0·55 to 1·1 lbs. on the same dynamometer, and a sample of the results with the greatest load that could be safely employed is appended:

Gramme Armature } Motor, 8·03 lbs.
Siemens' Field-Magnet }

Revolutions per minute.	Current in ampères.	Potential difference at terminals in volts.	Load on dynamo-meter in lbs.	Horse-power given in.	Horse-power given out.	Efficiency.
803	16·8	8·9	1·1	0·201	0·0176	0·0876
1,200	16·8	10·0	...	0·226	0·0263	0·147
1,953	17·1	12·0	...	0·229	0·0428	0·187
2,853	16·9	13·8	...	0·314	0·0625	0·199

Resistance of this motor when warm was 0·443 ohm.

The Jablochkoff motor, weighing 20·9 lbs., was the next tested, and the following results were obtained with the largest load that could be employed with a safe current:—

Jablochkoff Motor, 20·9 lbs.

Revolutions per minute.	Current in ampères.	Potential difference at terminals in volts.	Load on dynamo-meter in lbs.	Horse-power given in.	Horse-power given out.	Efficiency.
282	8·9	20·5	0·88	0·189	0·00494	0·0261
405	7·0	22·8	...	0·199	0·00711	0·0358
609	8·1	26·4	...	0·286	0·0107	0·0373
680	8·9	29·2	...	0·347	0·0119	0·0343
780	9·6	31·8	...	0·408	0·0137	0·0335

Resistance of this motor when warm was 1·17 ohms.

This motor gave, as will be seen, very small results, both as regards the power given out and the efficiency. This arises from the fact that this motor, whether regarded from an electric or from a mechanical point of view, is extremely defective in construction. When furnished to us the armature and field-magnets were in parallel circuit, but as, from the construction of the motor, during every revolution there are two places where the armature is entirely short-circuited by the brushes, it follows that this arrangement in parallel circuit is very bad. We therefore joined it up in series for our tests, but even then its bad construction, mechanically and electrically, prevented it doing more than a small amount of work, and doing it very economically.

As to the mechanical defects, the Jablochkoff motor consists of two circular coils, like the coils of a tangent galvanometer, placed at an angle of 45° with one another, while a third circular coil, having a slightly smaller diameter than the two fixed coils, revolves inside them, with its axis making an angle of 45° with the axis of rotation. As the moment of inertia of the revolving coil is very considerable, there is a large couple due to the rotation, tending to diminish the angle between the axis of the coil and the axis of rotation, and this torque is so large that even at speeds of 700 revolutions we found it necessary to strengthen the mechanical connections of this coil with the axis to prevent its being pulled out of position.

Sir Wm. Thomson has lately drawn attention to the importance of placing the axis of dynamo machines on board ship parallel with the axis of the ship about which it rolls, in order to prevent undue strain being put on the bearings of the running dynamo machine when the ship rolls. But the Jablochkoff motor is so made as to put the maximum strain on the shaft when rotating.

The next motor was one with a Gramme ring and Siemens' field-magnet, weighing 30·8 lbs. It was of the same general construction as the Gramme motor previously tried, but not quite the same in shape—that is, the proportions of the various parts were not the same. With this larger Gramme motor various loads and various speeds were employed, as in the previous cases, the greatest load that could safely be used being 0·88 lbs., the circumference of the compensating dynamometer employed in experimenting with this motor being 33·3 centimètres, and larger than before. The following is a sample of the results so obtained:—

GRAMME RING
SIEMENS' FIELD-MAGNET } MOTOR, 30·8 LBS.

Revolutions per minute.	Current in ampères.	Potential difference at terminals in volts.	Load on dynamo-meter in lbs.	Horse-power given in.	Horse-power given out.	Efficiency.
932	5·4	12·8	0·88	0·0920	0·0272	·296
1,543	6·3	20·2	...	0·170	0·0451	·265
2,527	6·7	28·4	...	0·255	0·0738	·289
3,134	7·3	34·5	...	0·339	0·0916	·270
3,252	7·7	38·4	...	0·395	0·0951	·241
4,117	8·7	45·6	...	0·528	0·120	·226

Resistance of this motor when warm was 0·978 ohms.

The results given in this paper, combined with some thousands of experiments made on our normal-size motor, have led us, since reading this paper, to the means of effecting an important improvement in the direction of diminishing the magnetic friction in these motors; and, by so doing, we have been enabled to increase the power from 0·25 to 0·30 horse, and the efficiency from 30 to 40 per cent.

The following is a sample of the results obtainable with our 37 lbs. motor, with its field-magnet made of the latest shape, and from which it will be seen that its power and efficiency both far exceed anything we have yet obtained with any other motor of about the same weight.

In the following experiments the current, and not the load, was kept constant, as we were desirous of seeing what the motor would do at various speeds when heated to the same amount.

SOME RESULTS OBTAINED WITH AYRTON AND PERRY'S NORMAL-SIZE MOTORS, WEIGHING 37 LBS.

The circumference of the circle formed by the axis of the cord of the dynamometer used in these experiments, and shown in

Fig. 28, was 3·13 feet. The columns headed W and W_1 give the weights in pounds in the two scale pans of Fig. 28.

Revolutions per minute.	Current in ampères.	Potential difference at terminals in volts.	W.	W_1.	$W - W_1$.	Horse-power given in.	Horse-power given out.	Efficiency.
720	25·9	12·6	2·375	·375	2·0	·441	·14	·317
1,570	25·3	19·0	2·062	·375	1·687	·645	·241	·373
1,710	25·3	21·3	2·000	·375	1·625	·727	·266	·365
2,005	25·5	25·1	2·000	·438	1·562	·860	·297	·345
			Lead of brushes now changed.					
860	21·7	12·0	1·937	·375	1·562	·350	·127	·363
1,570	21·7	17·5	1·687	·312	1·375	·401	·204	·401
1,190	21·5	14·1	1·812	·375	1·437	·407	·162	·397
1,880	21·2	19·8	1·500	·288	1·212	·563	·215	·383

The resistance of the motor when hot was 0·2 ohms.

The following gives a résumé of the best working results as to power and efficiency that we have obtained with motors weighing from 2 to 500 lbs:—

Name of motor.	Weight of motor in lbs.	Horse-power actually given out.	Revolutions per minute.	Efficiency.
Griscom	2·5	0·015	2,500	0·13
Gramme armature ... Siemens' field-magnet	8·03	0·0625	2,853	0·199
Jablochkoff	20·9	0·0137	780	0·0335
Gramme armature ... Siemens' field-magnet	30·8	0·0738 0·120	2,527 4,117	0·289 0·226
Ayrton and Perry ...	37·	0·3 0·2	2,000 1,570	0·34 0·40
De Meritens	72·	0·75	2,000	0·500
Siemens	519·	4·96 5·60	906 731	0·746 0·714

We have to thank several of our students for valuable assistance rendered us during the course of this investigation; and our thanks are especially due to Mr. Tomlinson for his indefatigable exertions.

The PRESIDENT: I am very sorry that we have not more time at our disposal to-night, particularly to go into discussion on so very important a paper, but we have a ballot. Professors Ayrton and Perry have told you of some apparatus they wish you to see; and, as it is now 10 o'clock, I am afraid we shall not be able to take any discussion to-night. I might also say that our next meeting will be the last meeting before the summer recess, and on that night Professor Hughes will bring before us a paper upon Magnetism. Therefore I must ask you at once to give a hearty vote of thanks to Professors Ayrton and Perry for their valuable paper.

I should like, however, just to ask Professor Ayrton whether the weight of dynamos he mentioned as giving a certain result was the total gross weight,—that is, the weight of the iron frame, copper and iron magnet,—or whether it simply included the copper and the magnet; because I think that would make a difference in the results.

Professor W. E. AYRTON, F.R.S.: The weight mentioned is the total weight of the armature, including base plate and anything that is supplied with the motor when purchased. The horse-power that I wrote down on the board is the horse-power given out effectively by the motor: the efficiency does not enter into that number. Efficiency tells you what the horse-power put into the motor must be when you know the horse-power given out. I mention that because it has just been stated to me that the steam-engine had beaten motors. I think that is a mistake. Large steam-engines may possibly have beaten small electro-motors, just as large steam-engines have beaten small steam-engines; but small steam-engines have not yet beaten small motors. I know of no small steam-engine which will give one horse-power per 100 lbs. of dead weight, inclusive of boiler, coal, and everything which must go with the engine. But this is accomplished in a motor.

On the motion of the President, a hearty vote of thanks was unanimously accorded to Professors Ayrton and Perry for their paper.

A ballot then took place, at which the following were elected:—

As Foreign Members:

D. H. Bates.

Emile Baudot.

As Member:

Edward Hopkinson, D.Sc.

As Associates:

Frank Bailey.
Harry Bird.
John Bolton.
Henry William Borns, Ph.D., F.C.S.
R. Heywood Claxton.
Edward Gardner Colton.
John Tudor Crome.
Robert Darby.
G. Dudley.
Commander Douglas Methuen Forsyth, R.N.
C. R. Heap.
Lieut. H. Capel L. Holden, R.A.
Captain Richard Hare Home, R.A.
Ernest Arthur Kennedy.
John Lavender, jun.
James P. MacGreggor.
Kenneth MacIver.
Earnest G. Pink.
C. H. Pownall.
Sidney Hugh Smith.
W. C. Smythe.
Henry John Staples.

As Students:

A. P. Chattock.
Henry Justus Eck.
John Peter Gorton.

The meeting then adjourned until Thursday evening, 24th May, 1883.

The One Hundred and Twenty-fourth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 24th, 1883—Mr. Willoughby Smith, President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The names of new candidates were announced, and, on the proposition of the President, the meeting decided to ballot for the new members at the close of the present meeting, in preference to the usual practice of waiting until the following meeting, seeing that the summer vacation would intervene.

The following paper was then read:—

THE CAUSE OF EVIDENT MAGNETISM IN IRON, STEEL, AND OTHER MAGNETIC METALS.

By Professor D. E. Hughes, F.R.S., Vice-President,

The extreme sensitiveness of the induction balance to all molecular changes in the structure of metals was remarked in my first paper on this subject to the Royal Society;[*] and in the case of iron and steel it is most remarkable, as the addition or subtraction of $\frac{1}{500000}$ part, or the addition of the smallest iron filing to an already large balanced mass of iron, is at once rendered evident and measureable.

Possessing such an invaluable instrument of research, I was desirous of investigating the molecular construction of iron and steel, but at once I met with a difficulty, viz., that magnetism itself completely changed the character of any piece of iron under investigation; consequently, finding no help or explanation of the effects produced from any accepted theories of magnetism, I was forced to investigate, by means of the induction balance, the whole question of magnetism as existing in the interior of a magnet, and to determine the particular structure for each case, such as neutrality and polarity.

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[*] "On an Induction Current Balance, and Experimental Researches made therewith."—*Proceedings Royal Society*, March 29, p. 56, 1879.

In a recent paper to the Royal Society, upon the theory of magnetism,* I described the use of, and demonstrations obtained by, the induction balance. In this paper I propose to confine myself to demonstrations that can be repeated without it, and whose effects can be observed by the aid of ordinary magnetic direction needles.

That magnetism is of a molecular nature has long been accepted, for it is evident that, no matter how much we divide a magnet, we still have its two poles in each separate portion, consequently we can easily imagine this division carried so far, that we should at last arrive at the molecule itself possessing its two distinctive poles, consequently all theories of magnetism attempt some explanation of the cause of this molecular polarity, and the reason for apparent neutrality in a mass of iron.

Couloumb and Poisson assume that each molecule is a sphere containing two distinct magnetic fluids, which in the state of neutrality are mixed together, but when polarised are separated from each other at opposite sides; and, in order to explain why these fluids are kept apart as in a permanent magnet, they had to assume, again, that each molecule contained a peculiar coercive force, whose functions were to prevent any change or mixing of these fluids when separated.

There is not one experimental evidence to prove the truth of this assumption; and as regards coercive force, we have direct experimental proof opposing this view, as we know that molecular rigidity or hardness, as in tempered steel, and molecular freedom or softness, as in soft iron, fulfil all the conditions of this assumed coercive force.

Ampère's theory, based upon the analogy of electric currents, supposes elementary currents flowing around each molecule, and that in the neutral state these molecules are arranged hap-hazard in all directions, but that magnetisation consists in arranging them symmetrically.

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* *Proceedings Royal Society*, May 10, 1883.

The objections to Ampère's theory are numerous. 1st. We have no knowledge or experimental proof of any elementary electric currents continually flowing without any expenditure of energy. 2nd. If we admit the assumption of electric currents around each molecule, the molecule itself would then be electro-magnetic, and the question still remains, What is polarity? Have the supposed electric currents separated the two assumed magnetic fluids contained in the molecule, as in Poisson's theory? or are the electric currents themselves magnetic, independent of the iron molecule?

In order to produce the supposed heterogeneous arrangement of neutrality, Ampère's currents would have either to change their position upon the molecule, and have no fixed axis of rotation, or else the molecule, with its currents and polarities, would rotate, and thus be acting in accordance with the theory of De la Rive. 3rd. This theory does not explain why (as in the case of soft iron) polarity should disappear whenever the exciting cause is removed, as in the case of transient magnetisation. It would thus require a coercive force in iron to cause exactly one-half of the molecules to instantly reverse their direction, in order to pass from apparent external polarity to that of neutrality.

The influence of mechanical vibrations and stress upon iron in facilitating or discharging its magnetism, as proved by Matteucci, 1847, in addition to the discovery by Page, 1837, of a molecular movement taking place in iron during its magnetisation, producing audible sounds, and the discovery by Dr. Joule, 1842, of the elongation of iron when magnetised, led De la Rive, in his remarkable "Treatise on Electricity," 1853, to give his theoretical views upon magnetism in the following remarkable words:—

"The whole of the magnetic molecular phenomena that we have been studying lead us to believe that the magnetisation of a body is due to a particular arrangement of its molecules, originally endowed with magnetic virtue, but which in the natural state are so arranged that the magnetism of the body that they constitute is not apparent. Magnetism would therefore consist in disturbing this state of equilibrium, or in giving to the particles an arrangement that makes manifest the property with which they are endowed, and not in developing it in them. The coercitive force should be the resistance of the molecules to change their relative positions."

Wiedermann, in 1861, gives a theory in which he admits the fluids of Poisson, or the elementary currents of Ampère, as the cause of polarity of the molecule, but believes that the molecules are turned in a general direction in the case of polarity, and that in neutrality, like Ampère's the magnetic axes of the molecules are turned in all directions.

Maxwell, in his remarkable treatise on "Electricity and Magnetism," 1881, page 75, gives the following résumé of Weber's theory:—

"Weber's theory differs from Poisson's, in assuming that the molecules of the iron are always magnets, even before the application of the magnetising force, but that in ordinary iron the magnetic axes of the molecules are turned indifferently in every direction, so that the iron, as a whole, exhibits no magnetic properties." And again, page 429, Maxwell says he agrees with Weber's views, and that neutrality, or unmagnetised iron, has the axes of its molecules placed indifferently in all directions, and that the act of magnetisation consists in turning all the molecules so that their axes are either rendered all parallel to one direction, or at least deflected in that direction.

I have quoted these several theories which admit of the inherent polarity of the molecule, and in that respect they entirely agree with my own; but the induction balance at once shows that they are erroneous in the most important part, for my researches have proved that neutrality is perfectly symmetrical, that there is no case of neutrality where the axes of the molecules are turned indifferently in all directions, and that we cannot obtain perfect neutrality except when the molecules form a complete closed circuit of attraction.

I believe that a true theory of magnetism should admit of complete demonstration, that it should present no anomalies, and that all the known effects should at once be explained by it.

From numerous researches I have gradually formed a theory of magnetism entirely based upon experimental results, and these have led me to the following conclusions:—

1. That each molecule of a piece of iron, steel, or other magnetic metal is a separate and independent magnet, having its two poles and distribution of magnetic polarity exactly the same as its total evident magnetism when noticed upon a steel bar-magnet.

2. That each molecule, or its polarity, can be rotated in either direction upon its axis by torsion, stress, or by physical forces such as magnetism and electricity.

3. That the inherent polarity or magnetism of each molecule is a constant quantity like gravity; that it can neither be augmented nor destroyed.

4. That when we have external neutrality, or no apparent magnetism, the molecules or their polarities arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction.

5. That when magnetism becomes evident, the molecules or their polarities have all rotated symmetrically in a given direction, producing a north pole if rotated in that direction as regards the piece of steel, or a south pole if rotated in the opposite direction. Also, that in evident magnetism, we have still a symmetrical arrangement, but one whose circles of attraction are not completed except through an external armature joining both poles.

6. That we have permanent magnetism when the molecular rigidity, as in tempered steel, retains them in a given direction, and transient magnetism whenever the molecules rotate in comparative freedom, as in soft iron.

Experimental Evidences.

In the above theory the coercive force of Poisson is replaced by molecular rigidity and freedom; and as the effects of mechanical vibrations, torsion, and stress upon the apparent destruction and facilitation of magnetism is well known, I will, before demonstrating the more serious parts of the theory, cite a few experiments to prove that molecular rigidity fulfils all the requirements of an assumed coercive force.

The influence of vibrations, torsion, or stress of any kind upon a magnetised steel or iron rod may be seen by striking with a wooden mallet rods of hard and soft steel, also hard and soft iron previously magnetised to a known degree. The tempered steel, owing to its molecular rigidity, will lose but 5 per cent., the soft steel 60, hard iron 50, and soft Swedish iron 99 per cent. of its magnetism, the amount of loss depending not so much upon whether the metal be steel or iron, as upon its degree of hardness and softness; and as hard steel requires far more power to magnetise it to the same force than iron, it is possible to imagine a steel so hard that its molecules could not rotate, and that consequently no magnetism could be manifested from a given inducing cause, whilst a perfectly soft iron would give the maximum effect, and instantly return to its previous state. From this we might in error suppose that soft Swedish iron could not retain its magnetism, and that its natural state would be zero, or neutrality. The apparent disappearance of magnetism, however, is here due to the extreme freedom of motion of its molecules allowing them at once to follow the comparatively feeble directing force of the earth's magnetism. We can demonstrate this by feebly magnetising a rod of soft iron held vertically, so that its north pole is at the lower portion. Upon removing the inducing magnet, or electro-magnetic coil, we find that the rod retains a powerful north polarity; but if magnetised in a contrary sense, then we have only *traces* of magnetism left upon the withdrawal of the inducing cause. To succeed in this experiment, as in all others where soft iron is mentioned, we should use the best Swedish charcoal iron, thoroughly annealed at high temperature.

We find, again, that rods of steel or iron will lose far less magnetism when vibrated in the magnetic dip, or vertically, when their north poles are at the lowest extremity, than when horizontal, or still less than when their poles are contrary to those of the earth's field, and also that they will acquire their maximum magnetism from a given exciting cause when held vertically as described, and the molecules allowed greater freedom of motion to obey the directing influence by vibrations, torsion, stress, or blows upon the iron. Any influence that would tend to give greater freedom of motion, such as heat or mechanical trepidations, gives a far higher magnetic force to the iron than could be obtained without these aids.

In order to render visible the effects of motion upon magnetism, we may take two glass tubes, or ordinary phials, of any length or diameter, say, 10 centimètres in length by 2 centimètres in diameter. If we now put iron filings in these tubes, leaving about one-third vacant, so as to allow complete freedom in the filings when shaken, we find that each tube, when magnetised, retains an equal amount of residual magnetism, and that this all disappears upon slightly shaking the tube. We are thus imitating the effects of vibration. But if in one of these tubes we pour melted resin (in fact, any slightly viscous liquid, such as petroleum, suffices), we then render these filings more rigid, and then we can no longer produce by shaking, the disappearance of its residual magnetism. In pouring in petroleum we have apparently been introducing a strong coercitive force, but we know that it can only have the mechanical effect of rendering the iron filings less free to turn, and so comparatively rigid. If we desire to see the effect of torsion, we have only to shake the filings so that when the tube is held horizontally the vacant space is above, and rotate it slighty (but without shaking) about a horizontal axis. Its remaining magnetism instantly disappears upon rotation, although we evidently have not changed the longitudinal position of its particles. A similar effect takes place upon a soft iron rod, for if we magnetise it and observe its remaining magnetism, we find that, upon giving a slight torsion to this wire. its remaining magnetism instantly disappears—a similar effect to that in the rotating tube of iron filings. But if the iron is rendered more rigid by hammering, or steel rendered hard and rigid by tempering, torsions or vibrations have but little effect, as in the case of the filings rendered rigid as above mentioned. Thus we have no longer need of an assumed mysterious coercive force to account for the retention of magnetism, for once knowing the mechanical qualities of iron and steel, and their degree of molecular rigidity or hardness, we can at once predict their retentive magnetic powers.*

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* "On the Molecular Rigidity of Tempered Steel," by Professor D. E Hughes, F.R.S.—*Proceedings Institution of Mechanical Engineers*, pages 72-79 Jan., 1883.

Rotation of Inherent Polarised Molecules.

Torsion, as well as mechanical vibrations, has, as we have seen, a powerful influence in aiding the molecules to overcome their inertia, and thus aid them to rotate in the direction of the inducing influence; and we may thus polarise strongly a flat soft iron rod by simply bending or vibrating it when held vertically, and if we measure the magnetic force obtained we shall notice that the force is strictly relative to the degree of softness of the iron. Thus, with hard steel we should obtain only *traces* of polarisation, whilst with extremely pure soft Swedish iron we obtain the maximum of force. The bar of iron or steel, being held in the earth's magnetic field, of infinite size compared with the bar, and infinitely homogeneous, cannot deflect or weaken its surrounding field. Its lower portion being north, apparently strengthens it by its reaction, whilst its upper, south, apparently weakens the field; but, as Maxwell has shown, "the two poles of each molecule are equal and opposite, consequently the sum of each molecule and the whole mass must be zero."

We have a far greater induced polarity in iron or steel when the iron is in thin bars or small wires, and this we should expect, as the external molecules rotate directly under the influence of the earth's magnetism, whilst those forming the interior of the bar either rotate feebly, or, as in the case of very thick bars, actually act as an armature, preventing, by their influence, free rotation of the exterior molecules.

Thus, as the sum of the two and equal polarities in a bar of iron is zero, it is evident that its polarity must be inherent. I have some remarkably pure soft Swedish iron wire, one millimètre in diameter, and as its inherent polar force seemed great when held vertically in the earth's magnetic field, I measured in the induction balance this force compared with a similar column of the magnetic atmosphere which it displaced. The inherent polarity of this wire, simply rendered evident by the earth's magnetism, was 15,600 times greater than the column it displaced.

We cannot, either by induction, conduction, or concentration, produce a greater force in another body of similar displacement or size, otherwise we could easily create power from a feeble source.

Thus the enormously greater magnetic power observed in iron than the same column of air which it displaces must be due to the *inherent* polarity of its molecules.

Amongst numerous bars of iron upon which I have experimented, one of ordinary hoop-iron, 2 centimètres wide, 40 centimètres long, and 1½ millimètre thick, not softened, possesses sufficient molecular rigidity to be apparently uninfluenced by earth's magnetism. When this rod is rendered neutral, we have but feeble polarity—mere traces when it is held vertically under the earth's magnetic influence; but if we apply a few successive torsions or vibrations to it when thus held, we have at once several thousand times greater polarity than before. Now, if iron had the power of deflecting or concentrating the earth's magnetism upon itself, it should not require the mechanical aid to molecular rotation given to it by these torsions or vibrations. Thus we are forced to conclude at least the existence of the inherent polarity of the molecules; and, if we admit this, we must also, as a necessary consequence, admit the rotation of these molecules, else we cannot explain why mechanical vibrations allowing freedom of motion should always produce the polarity in accordance with the directing cause. I have already shown that torsion and vibrations *per se* are apparently destructive of magnetism; consequently in this case Poisson's two fluids and Ampère's parallel currents should, according to their theory, be mixed or heterogenous, whilst according to the views I am sustaining the polarised molecules should obey, as compass needles, any magnetic directing cause whenever sufficient molecular freedom of motion allows free rotation.

The inherent polarity of iron may again be observed by drawing a flat rod of soft iron over one or both poles of a permanent magnet. This rod will then be powerfully magnetised, its remaining magnetism, when separated from the magnet, being sufficiently powerful to strongly deflect a suspended direction needle. A few slight torsions or vibrations will then completely discharge it. Now, suppose this operation repeated successively many thousand times, if there was no inherent polarity we should have gradually drawn all the polarity out of the magnet, and discharged it into the atmosphere. Nothing of the kind takes place. The molecules of the iron are simply rotated each time, and the only energy in work expended or lost comes from the arm of the experimenter, and the energy required would be strictly in accordance with the molecular freedom, or softness and hardness of the iron and steel: thus, whilst soft iron could be easily polarised and discharged by mechanical torsions, hard-tempered steel would require a far greater amount.

Dr. Warren de la Rue, F.R.S., kindly aided me in this part of the research by passing a current from his well-known chloride of silver battery through iron and steel wires. A condenser of 42·8 microfarad capacity, charged by 3,360 cells, was used. We passed this enormous electric charge longitudinally through the wires, and observations were made as to whether any change whatever was produced in their quality or inherent polarity, the result being that these wires gave exactly the same magnetic polarity from a given directing or inducing cause as before, being similar in nature and degree, consequently this enormous electric force had not changed or destroyed the original inherent polarity.

If the molecules possess inherent polarity and rotate upon their axes, similar to a series of compass needles having a slight degree of frictional rigidity, then, upon passing one pole of a magnet above them, they would turn symmetrically in one direction, and drawing the same pole of the magnet in the contrary direction would rotate them, and they would then remain symmetrically in the opposite direction.

A precisely similar effect takes place in a soft iron rod, placed east and west a few inches above a direction needle. Upon drawing the south pole of a powerful natural magnet at a few centimètres distance above the wire from east to west, the north polarities of the molecules successively turn in the direction of west, following the attraction of the south pole, as previously seen on the small compass needles. The rod is now magnetised with its north pole west, as indicated by the direction needle below any portion of this rod. Upon passing the same south pole of the natural magnet in a contrary direction, the molecules all rotate, their north poles still turning successively to the south pole of the permanent magnet until its arrival at the end from which the first magnetisation commenced. The rod has now entirely changed its polarity, and its north pole is east.

This phenomenon is well known in the ordinary magnetisation of rods, where care is taken to draw the magnet always in a similar direction, or the poles would be reversed at each to and fro drawing. To account for this, on Couloumb-Poisson's theory, it would be requisite that, first, all the fluids be separated with their north fluids symmetrically in one direction, but on drawing back the magnet, these fluids would have to mix together, the north fluid passing through its south fluid to be finally opposite to its previous position, its coercive force doing the double work of allowing both fluids to mix and pass through each other, and finally keeping them entirely apart. Ampère's theory would require that from a hap-hazard arrangement the molecules should become symmetrically arranged upon the first passage of the magnet, then upon its reversed direction one half of the electric elementary currents should successively revolve in a contrary direction to arrive at neutrality before, finally, the other half followed the direction of the first half, and now all these currents would be revolving in the opposite direction to that upon the first magnetisation. We thus see that both these theories, whilst resting altogether upon assumption, are extremely complicated and improbable.

We might suppose, from the theory which I am advocating, that upon the rotation of the molecules there would be some disturbance or mechanical trepidation; and such is found to be the case, as first observed by Page, and afterwards verified by Dr. Joule and De la Rive, in the molecular sounds produced in iron upon its magnetisation. Reis's first telephone was founded upon these sounds, and Du Moncel has made numerous researches upon this subject.

In the last of my experiments cited, the sounds are too feeble to be heard, but by the application of the microphone these trepidations at once become audible.

That molecules of iron and other metals rotate with time, whose period becomes shortened by mechanical vibrations, is well known in metallurgy, the ultimate result being generally the passage from a fibrous condition, as in iron wires, to a high degree of crystallisation. For many years I employed a circular vibrating spring as the regulator of speed of my printing telegraph instrument, and although this spring was so regulated by means of a frictional break, or "Frein," as not to surpass its limits of elasticity, these springs were constantly breaking after a few days' use, and, as a matter of urgent necessity, I made special researches into the cause of this breaking after a few days constant vibratory action. I found at the point of rupture a high state of crystallisation. Fibrous iron would thus become thoroughly crystallised and break in one day; the number of vibrations for an instrument in constant use during 24 hours being 1,209,600. Thus we could roughly estimate the life of iron in the form of one of these springs at one million vibrations. Copper crystallised in one hour, and all metals and alloys were inferior to steel, except aluminium bronze. The latter springs would stand six weeks' constant use, or some fifty millions of vibrations. I finally resolved this problem by spreading the amount of vibrating work over a spiral spring containing 3 mètres of steel rod wound into the same space as previously held by the straight rod of 30 centimètres; by this means the average life of these springs has become five years. Evidently the molecules of these fibrous springs must have rotated under the vibrations, in order to produce crystals. The same phenomenon is observed in axles of carriages receiving constant trepidations, large crystals being always found at the point of fracture. Again, if we rapidly magnetise and demagnetise an iron rod, we have the production of evident heat, due to the constant motion of its molecules.

Maxwell describes an experiment of Beetz, in which an exceedingly small filament of iron was deposited by electrotype, under the influence of a strong magnetic field, in order to arrive at the inherent polarity of comparatively few molecules, and, as its magnetic force was very great, he regards the experiment as conclusive. My own experiments show that we have far less external magnetic force from a solid bar than from a thin tube or flat bar of the same surface exposed to a limited exciting cause. We know that magnetism does not penetrate to a very great depth, and we also know that, if to a thin steel permanent magnet we place another piece unmagnetised, or, better still, a rod of soft iron, its external polarity is greatly reduced; consequently the external evidence of polarity is not a direct measure of the degree of rotation, nor of the total inherent polarity of its mass. We may have a great superficial *external* rotation superposed upon rotations of an opposite nature, as will be seen later; and thus the internal molecules of a magnet often act more or less as an external armature in closing its circle of attractions.

I have stated my belief that the molecule itself possesses its inherent polarity, which, like gravity, is an endowed quality for which we have no more reason to suspect the cause to be elementary electric currents than that elementary currents should be the cause of gravity, chemical affinity, or cohesion and its polar power of crystallisation, most of which are affected by an electric current. We have a certain analogy between electric currents and magnetism, but not so great as the analogy between the magnetic polarity of a molecule and its other endowed qualities.

Magnetism, like chemical affinity, cohesion, and crystallisation, has its critical points. Faraday discovered that at red-yellow heat iron instantly lost its apparent polar magnetic power, to be as instantly restored at red heat, the critical point varying in iron, steel, etc., and being the lowest in nickel. This would be difficult to explain upon Ampère's theory, as we should have to admit the instant destruction or cessation of the elementary currents, to be again restored at a few degrees less temperature. It would be equally difficult to explain under my view, if it did not belong to a whole class of phenomena due to the possession by the molecules of various endowed qualities, of which chemistry and all our means of research can only teach us their critical points, without attempting to explain why for instance, iron has a greater affinity for oxygen than gold. We know that it is so; we know that the molecules of all matter are endowed with certain qualities having certain critical points, and I can see no reason for separating their magnetic inherent polarity from their numerous other qualities.

Neutrality.

The apparatus needed for researches upon evident external polarity requires no very great skill or thought, but simply an apparatus to measure correctly the force of the evident repulsion or attraction; in the case of neutrality, however, the external polarity disappears, and we consequently require special apparatus, together with the utmost care and reflection in its use.

From numerous researches previously made by means of the induction balance, the results of which I have already published, I felt convinced that in investigating the cause of magnetism and neutrality I should have in it the aid of the most powerful instrument of research ever brought to bear upon the molecular construction of iron, as indeed of all metals. It neglects all forces which do not produce a change in the molecular structure, and enables us to penetrate at once to the interior of a magnet or piece of iron, observing only its peculiar structure and the change which takes place during magnetisation or apparent neutrality.

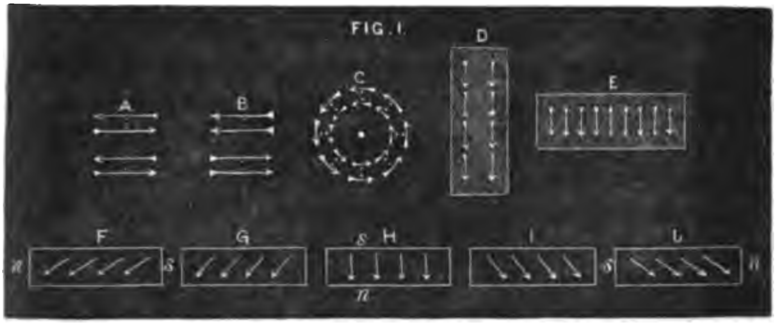


FIG. I.

The induction balance is affected by three distinct arrangements of molecular structure in iron and steel, by means of which we have apparent external neutrality.

Fig. 1 shows several polar directions of the molecules as indicated by the arrows. Poisson assumed, as a necessity of his theory, that a molecule is spherical, but Dr. Joule's experimental proof of the elongation of iron by $\frac{1}{720000}$ of its length when magnetised, proves at least that its form is not spherical; and, as I am unable at present, to demonstrate my own views as to its exact form, I have simply indicated its polar direction by arrows

—the dotted oval lines merely indicating its limits of free elastic rotation.

In Fig. 1, at A, we have neutrality by the mutual attraction of each pair of molecules, being the shortest path in which they could satisfy their mutual attractions. At B we have the case of superposed magnetism of equal external value, rendering the wire or rod apparently neutral, although a lower series of molecules are rotated in the opposite direction to the upper series, giving to the rod opposite and equal polarities. At C we have the molecules arranged in a circular chain around the axis of a wire or rod through which an electric current has passed. At D we have the evident polarity induced by the earth's directive influence when a soft iron rod is held in the magnetic meridian. At E we have a longitudinal neutrality produced in the same rod when placed magnetic west, the polarity in the latter case being transversal.

In all these cases we have a perfectly symmetrical arrangement, and I have not yet found a single case in well-annealed soft iron in which I could detect a heterogeneous arrangement, as supposed by Ampère, De la Rive, Weber, Wiedermann, and Maxwell.

We can only study neutrality with perfectly soft Swedish iron. Hard iron and steel retain previous magnetisations, and an apparent external neutrality would in most cases be the superposition of one magnetism upon another of equal external force in the opposite direction, as shown at B, Fig. 1. Perfectly soft iron we can easily free, by vibrations, from the slightest trace of previous magnetism, and study the neutrality produced under varying conditions.

If we take a flat bar of soft iron, of 30 or more centimètres in length, and hold it vertically (giving while thus held a few torsions, vibrations, or, better still, a few slight blows with a wooden mallet, in order to allow its molecules to rotate with perfect freedom), we find its lower end to be of strong north polarity, and its upper end south. On reversing the rod and repeating the vibrations, we find that its lower end has precisely a similar north polarity. Thus the iron is homogeneous, and its polarity symmetrical. If we now magnetise this rod to produce a strong south pole at its lower portion, we can gradually reverse this polarity, by the influence of earth's magnetism, by slightly tapping the upper extremity with a small wooden mallet. If we observe this rod by means of a direction needle at all parts, and successively during its gradual passage from one polarity to the other, there will be no sudden break into a hap-hazard arrangement, but a gradual and perfectly symmetrical rotation from one direction to that of the opposite polarity.

If this rod is placed east and west, having first, say, a north polarity to the right, we can gradually discharge or rotate the molecules to zero, and as gradually reverse the polarity by simply *inclining* the rod so as to be slightly influenced by earth's magnetism; and at no portion of this passage from one polarity to neutrality, and to that of the opposite name, will there be found a break of continuity of rotation or hap-hazard arrangement. If we rotate this rod slowly, horizontally or vertically, taking observations at each few degrees of rotation of an entire revolution, we find still the same gradual symmetrical change of polarity, and that its symmetry is as complete at neutrality as in evident polarity.

In all these cases there is no complete neutrality, the longitudinal polarity simply becoming transversal when the rod is east and west. F, G, H, I, J, Fig. 1, show this gradual change, H being neutral longitudinally, but polarised transversely. If, in place of the rod, we take a small square soft iron plate and allow its molecules freedom under the sole influence of the earth's magnetism, then we invariably find the polarity in the direction of the magnetic dip, no matter in what position it be held, and a sphere of soft iron could only be polarised in a similar direction. Thus we can never obtain complete external neutrality whilst the molecules have freedom and do not form an internal closed circle of mutual attractions; and whatever theory we may adopt as to the cause of polarity in the molecule, such as Couloumb's, Poisson's, Ampère's, or Weber's, there can exist no hap-hazard arrangement in perfectly soft iron, as long as it is free from all external causes except the influence of the earth; consequently these theories are wrong in one of their most essential parts.

We can, however, produce a closed circle of mutual attraction in iron and steel, producing complete neutrality as long as the structure is not destroyed by some stronger external directing influence.

Oersted discovered that an external magnetic needle places itself perpendicular to an electric current; and we should expect that, if the molecules of an iron wire possessed inherent polarity and could rotate, a similar effect would take place in the interior of the wire to that observed by Oersted. Wiedermann first remarked this effect, and it has been known as circular magnetism. This circle, however, consists really in each molecule having placed itself perpendicular to the current, simply obeying Oersted's law, and thus forming a complete circle in which the mutual attractions of the molecules forming that circle are satisfied, as shown at C, Fig. 1. This wire becomes completely neutral, any previous symmetrical arrangement of polarity rotating to form its complete circle of attractions; and we can thus form in hard iron and steel a neutrality extremely difficult to break up or destroy. We have evident proof that this neutrality consists of a closed chain, or circle, as by torsion we can partially deflect them on either side; thus from a perfect externally neutral wire, producing either polarity, by simple mechanical angular displacement of the molecules, as by right- or left-handed torsion.

If we magnetise a wire placed east and west, it will retain this polarity until freed by vibrations, as already remarked. If we pass an electric current through this magnetised wire, we can notice the gradual rotation of the molecules, and the formation of the circular neutrality. If we commence with a weak current, gradually increasing its strength, we can rotate them as slowly as may be desired. There is no sudden break or hap-hazard moment of neutrality: the movements to perfect zero are accomplished with perfect symmetry throughout.

We can produce a more perfect and shorter circle of attractions by the superposition of magnetism, as at B, Fig. 1. If we magnetise a piece of steel or iron in a given direction with a strong magnetic directing power, the magnetism penetrates to a certain depth. If we slightly diminish the magnetising power, and magnetise the rod in a contrary direction, we may reduce it to zero, by the superposition of an exterior magnetism upon one of a contrary name existing at a greater depth; and if we continue this operation, gradually diminishing the force at each reversal, we can easily superpose ten or more distinct symmetrical arrangements, and, as their mutual attractions are satisfied in a shorter circle than in that produced by electricity, it is extremely difficult to destroy this formation when once produced.

The induction balance affords also some reasons for believing that the molecules not only form a closed circle of attractions, as at B, but that they can mutually react upon each other, so as to close a circle of attractions as a double molecule, as shown at A. The experimental evidence, however, is not sufficient to dwell on this point, as the neutrality obtained by superposition is somewhat similar in its external effects.

We can produce a perfectly symmetrical closed circle of attractions of the nature of the neutrality of C, Fig. 6, by forming a steel wire into a closed circle, 10 centimètres in diameter, if this wire is well joined at its extremities by twisting and soldering. We can then magnetise this ring by slowly revolving it at the extremity of one pole of a strong permanent magnet; and, to avoid consequent poles at the part last touching the magnet, we should have a graduating wedge of wood, so that whilst revolving it may be gradually removed to greater distance. This wire will then contain no consequent points or external magnetism: it will be found perfectly neutral in all parts of its closed circle. Its neutrality is similar to C, Fig. 6; for if we cut this wire at any point we find extremely strong magnetic polarity, being magnetised by this method to saturation, and having retained (which it will indefinitely) its circle of attractions complete.

I have already shown that soft iron, when its molecules are allowed perfect freedom by vibration, invariably takes the polarity of the external directing influence, such as that of the earth, and it does so even with greater freedom under the influence of heat. Manufacturers of electro-magnets for telegraphic instruments are very careful to choose the softest iron and thoroughly anneal it;

but very few recognise the importance as regards the position of the iron whilst annealing it under the earth's directing influence. The fact, however, has long since been observed.

Dr. Hooke, 1684, remarked that steel or iron was magnetised when heated to redness and placed in the magnetic meridian. I have slightly varied this experiment by heating to redness three similar steel bars, two of which had been previously magnetised to saturation, and placed separately with contrary polarity as regards each other, the third being neutral. Upon cooling, these three bars were found to have identical and similar polarity. Thus the molecules of this most rigid material, cast steel, had become free at red heat, and rotated under the earth's magnetic influence, giving exactly the same force on each; consequently the previous magnetisation of two of these bars had neither augmented nor weakened the inherent polarity of their molecules. Soft iron gave under these conditions by far the greatest force, its inherent polarity being greater than that of steel.

I have made numerous other experiments bearing upon the question of neutrality, but they all confirm those I have cited, which I consider afford ample evidence of the symmetrical arrangement of neutrality.

Superposed Magnetism.

Knowing that by torsion we can rotate or diminish magnetism, I was anxious to obtain by its means a complete rotation from north polarity to neutrality, and from neutrality to south polarity, or to completely reverse magnetic polarity by a slight right or left torsion.

I have succeeded in doing this, and in obtaining strong reversal of polarities, by superposing one polarity given whilst the rod is under a right elastic torsion, with another of the opposite polarity given under a left elastic torsion, the neutral point then being reached when the rod is free from torsion. The rod should be very strongly magnetised under its first or right-hand torsion, so that its interior molecules are rotated, or, in other words, magnetised to saturation; the second magnetisation in the contrary sense and torsion should be feebler, so as only to magnetise the surface, or not more than one-half its depth: these can be easily adjusted to each other so as to form a complete polar balance of force, producing, when the rod is free from torsion, the neutrality as shown at B, Fig. 1.

The apparatus needed is simply a good compound horse-shoe permanent magnet, 15 centimètres long, having six or more plates, giving it a total thickness of at least 3 centimètres. We need a sufficiently powerful magnet, as I find that I obtain a more equal distribution of magnetism upon a rod or strip of iron by drawing it lengthwise over a single pole in a direction from that pole, as shown in Fig. 2; we can then obtain saturation by repeated drawings, keeping the same molecular symmetry in each experiment.

In order to apply a slight elastic torsion when magnetising rods or wires, I have found it convenient to attach two brass clamp keys to the extremities of the rods, or simply turn the ends at right angles, as shown in the following diagram, by which means we can apply an elastic twist or torsion whilst drawing the rod over the pole of the permanent magnet. We can thus superpose several and opposite symmetrical structures, producing a polar north or south as desired, greatly in excess of that possible under a single or even double magnetisation, and by carefully adjusting the proportion of opposing magnetisms, so that both polarities have the same external force, the rod will be at perfect external neutrality when free from torsion.

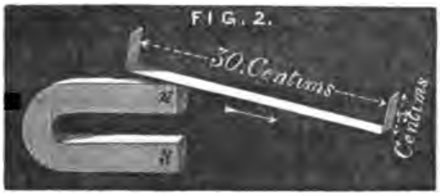


FIG. 2.

If we now hold one end of this rod at a few centimètres distance from a magnetic directive needle, we find it perfectly neutral when free of torsion, but the slightest torsion right or left at once produces violent repulsion or attraction, according to the direction of the torsion given to the rod, the iron rod or strips of hoop-iron which I use for this experiment being able, when at the distance of 5 centimètres from the needle, to turn it instantly 90° on either side of its zero.

The external neutrality that we can now produce at will is absolute, as it crosses the line of two contrary polarities, being similar to the zero of my electric sonometer, whose zero is obtained by the crossing of two opposing electric forces.

This rod of iron retains its peculiar powers of reversal in a remarkable degree, a condition quite different to that of ordinary magnetisation, for the same rod, when magnetised to saturation under a single ordinary magnetism, loses its evident magnetism by a few elastic torsions, as I have already shown; but when it is magnetised under the double torsion with its superposed magnetism, it is but slightly reduced by variations or numerous torsions, and I have found it impossible to render this rod again free from its double polar effects, except by strongly remagnetising it to saturation with a single polarity. The superposed magnetism then becomes a single directive force, and we can then by a few vibrations or torsions reduce the rod to its ordinary condition.

The effects of superposed magnetism and its double polarity I have produced in a variety of ways, such as by the electro-magnetic influence of coils, or in very soft iron simply by the directive influence of the earth's magnetism, reversing the rod and torsions when held in the magnetic meridian, these rods when placed magnetic west showing distinctly the double polar effects.

It is remarkable, also, that we are enabled to superpose and obtain the maximum effects on thin strips of iron from ¼ to ½ millimètre in thickness, whilst in thicker rods we have far less effect, being masked by the comparatively neutral state of the interior, the exterior molecules then reacting upon those of the interior, allowing them to complete in the interior their circle of attractions.

I was anxious to obtain wires which would preserve this structure against the destructive influence of torsion and vibrations, so that I could constantly employ the same wires without the comparatively long and tedious process of preparation. Soft iron soon loses the structure, or becomes enfeebled, under the constant to and fro torsions requisite where we desire a constant change of polarity, as described later in the magnetic bells. Hard steel preserves its structure, but its molecular rigidity is so great that we obtain but mere traces of any change of polarity by torsion. I have found, however, that fine cast drill steel, untempered, of the kind employed by watchmakers, is most suitable: these are generally sold in straight lengths of 30 centimètres. Wires 1 millimètre in diameter should be used, and when it is desired to increase the force, several of these wires, say, nine or ten, should be formed into a single rod or bunch.

The wire as sold is too rigid to give its maximum of molecular rotation effect. We must therefore give it two entire turns or twists to the right, and strongly magnetise it on the north pole of the magnet whilst under torsion. We must again repeat this operation in the contrary direction, after restoring the wire to its previous position, giving now two entire turns to the left and magnetising it on the south pole. On restoring the wire to its original place, it will be extremely flexible, and we may now superpose several contrary polarities under contrary torsions, as already described.

The power of these wires, if properly prepared, is most remarkable, being able to reverse their polarity under torsion, as if they were completely saturated; and they preserve this power indefinitely if not touched by a magnet. It would be extremely difficult to explain the action of the rotative effects obtained in these wires under any other theory than that which I have advanced; and the absolute external neutrality that we obtain in them when the polarities are changing, we know, from their structure, to be perfectly symmetrical.

I was anxious to show, upon the reading of this Paper, some mechanical movement produced by molecular rotation, consequently I have arranged two bells that are struck alternately by a polarised armature put in motion by the double polarised rod I have already described, but whose position, at 3 centimètres distant from the axis of the armature, remains invariably the same. The magnetic armature consists of a horizontal light steel bar suspended by its central axle; the bells are thin wine glasses, giving a clear. musical tone loud enough, by the force with which they are struck, to be clearly heard at some distance. The armature does not strike these alternately by a pendulous movement, as we may easily strike only one continuously, the friction and inertia of the armature causing its movements to be perfectly dead-beat when not driven by some external force, and it is kept in its zero position by a strong directive magnet placed beneath its axle.

The mechanical power obtained is extremely evident, and is sufficient to put the sluggish armature in rapid motion, striking the bells six times per second, and with a power sufficient to produce tones loud enough to be clearly heard in all parts of the hall of the Society. As this is the first direct transformation of molecular motion into mechanical movement, I am happy to show it on this occasion.

There is nothing remarkable in the bells themselves, as they evidently could be rung if the armature was surrounded by a coil, and worked by an electric current from a few cells. The marvel, however, is in the small steel superposed magnetic wire producing by slight elastic torsions from a single wire, 1 millimètre in diameter, sufficient force from mere molecular rotation to entirely replace the coil and electric current.

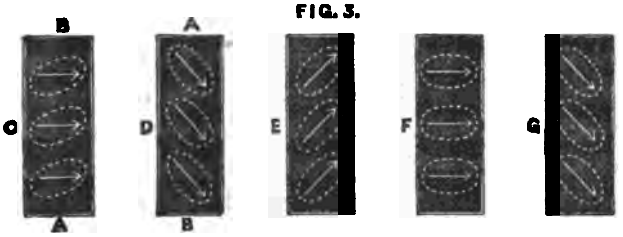
Elastic Nature of the Ether surrounding the Magnetic Molecules.

During these researches I have remarked a peculiar property of magnetism, viz., that not only can the molecules be rotated through any degree of arc to its maximum, or saturation, but that, whilst it requires a comparatively strong force to overcome its rigidity or resistance to rotation, it has a small field of its own through which it can move with excessive freedom, trembling, vibrating, or rotating through a small degree with infinitely less force than would be required to rotate it permanently on either side. This property is so marked and general that we can observe it without any special iron or apparatus.

Let us take a flat rod of ordinary hoop-iron, 30 or more centimètres in length. If, whilst holding this vertically, we give freedom to its molecules by torsions, vibrations, or, better still, by a few blows with a wooden mallet upon its upper extremity, we find, as is well known, that its lower portion is strongly north, and its upper south. If we reverse this rod, we now find it neutral at both extremities. We might here suppose that the earth's directing force had rotated the molecules to zero or transversely, which in reality it has done, but only to the limit of their comparatively free motion; for if we reverse the rod to its original position, its previous strong polarity reappears at both extremities, thus the central point of its free motion is inclined to the rod, giving by its free motion great symmetrical inclination and polarity in one direction, but when reversed the inclination is reduced to zero.

In Fig. 3, D shows the bar of iron when strongly polarised by earth's magnetic influence, under vibrations, with a sufficient force to have rotated its elastic centre of action. C shows the same bar with its molecules at zero, or transversal, the directing force of earth being insufficient without the aid of mechanical vibration to allow them to change. The dotted lines of D suppose the molecule to be in the centre of its free motion, whilst at C the molecules have rotated to zero, as they are prevented from further rotation by being at the extreme end of its free motion.

FIG. 3.



If, now, we hold the rod vertically, as at C, giving neutrality, and give a few slight blows with a wooden mallet to its upper extremity, we can give just the amount of freedom required for it to produce evident polarity, and we then have equal polarity, no matter which end of the bar is below, the centre of its free rotation here being perfect, and the rod perfectly neutral longitudinally when held east and west. If, on the other hand, we have given too much freedom by repeated blows of the mallet, its centre of free motion becomes inclined with the molecules, and we arrive at its first condition, except that it is now neutral at D and polarised at C. From this it will be seen that we can adjust this centre of action, by vibrations or blows, to any point within the external directing influence.

We can perceive this effect of free rotation in a limited space in all classes of iron and steel, being far greater in soft Swedish iron than in hard iron or steel. A similar phenomenon takes place if we magnetise a rod held vertically in the direction of earth's magnetism. It then gives greater polarity than if magnetised east or west, and if magnetised in a contrary sense to earth's magnetism, it is very feebly magnetised, or, if the rod is perfectly soft, it becomes neutral after strong magnetisation. This property of comparative freedom, and the rotation of its centre of action, can be demonstrated in a variety of ways. One remarkable example of it consists in the telephone. All those who are thoroughly acquainted with electro-magnetism, and know that it requires measurable time to charge an electro-magnet to saturation (about $\frac{1}{15}$ of a second for those employed in telegraphy), were surprised that the telephone could follow the slightest change of timbre, requiring almost innumerable changes of force per second. I believe the free rotation I have spoken of through a limited range explains its remarkable sensitiveness and rapidity of action, and, according to this view, it would also explain why loud sounding telephones can never repeat all the delicacy of timbre that is easily done with those only requiring a force comprised in the critical limits of its free rotation. This property, I have found, has a distinct critical value for each class of iron, and I propose soon to publish researches upon the molecular construction of steel and iron, in which I have made use of this very property as a guide to the quality of the iron itself.

The elastic rotation (in a limited space) of a molecule differs entirely from that known as mechanical elasticity. In perfectly soft iron we have feeble *mechanical* elasticity, whilst in tempered steel we have that elasticity at its maximum. The contrary takes place as regards *molecular* elasticity. In tempered steel the molecules are extremely rigid, and in soft iron its molecular elasticity is at its maximum. Its free motion differs entirely from that given it by torsion or stress. We may assume that a molecule is surrounded by continuous ether, more of the nature of a jelly than of that of a gas: in such a medium a molecule might freely vibrate through small arcs, but a rotation extending beyond its critical limit would involve a much greater expenditure of force.

The discovery of this comparatively free rotation of molecules, by means of which, as I have shown, we can (without in any degree disturbing the external mechanical elasticity of the mass) change the axes of their free motion in any direction desired, has led me into a series of researches which have only indirectly any relation with the theory of magnetism. I was extremely desirous, however, of finding an experimental evidence which in itself should demonstrate all portions of the theory, and the following experiment, I believe, answers this purpose:—

Let us take a square soft iron rod, 5 millimètres in diameter by 30 or more centimètres in length, and force the molecules, by aid of blows from a wooden mallet, as previously described, to have their centres of free motion in one direction, the rod will (as already shown) have polarity at both ends, when held vertically; but, if reversed, both ends become completely neutral.

If now we turn the rod to its first position, in which it shows strong polarity, and magnetise it whilst held vertically, by drawing the north pole of a sufficiently powerful permanent magnet from its upper to its lower extremity, we find that this rod, instead of having south polarity at its lower portion, as we should expect from the direction of the magnetisation, is completely neutral at both extremities, but if we reverse the rod, its fullest free powers of magnetisation now appear in the position where it was previously neutral. Thus, by magnetisation, we have completely rotated its free path of action, and find that we can rotate this path as desired in any direction by the application of a sufficient directing power.

If we take a rod as described, with its polarities evident when held vertically, and its neutrality also evident when its ends are reversed in the same magnetic field, we find that its polarity is equal at both ends, and that it is in every way symmetrical with a perfect magnet. If we *gradually* reverse the ends and take observations of its condition through each degree of arc passed over, we find an equal symmetrical diminution of evident external polarity until we arrive at neutrality, when it has no external trace of inherent polarity, but its inherent polarity at once becomes evident by a simple return to its former position. Thus the rod has passed through all the changes from polarity to neutrality, and from neutrality to polarity, and these changes have taken place with complete symmetry.

The limits of this paper do not allow me to speak of the numerous theoretical evidences as shown by the use of my induction balance. I believe, however, that I have cited already experimental evidences to show that what has been attributed to coercive force is really due to molecular freedom or rigidity; that in inherent molecular polarity we have a fact admitted by Couloumb, Poisson, Ampère, De la Rive, Weber, Du Moncel, Wiedermann, and Maxwell; and that we have also experimental evidence of molecular rotation and of the symmetrical character of polarity and neutrality.

The experiments which I have brought forward in this paper, in addition to those mentioned in my paper read before the Royal Society, will, I hope, justify me in having advanced a theory of magnetism which I believe in every portion allows at least experimental evidences of its probable truth.

The PRESIDENT: For the last 2,000 years man has been asking nature, What is magnetism? and endeavouring by every means in his power to solve the mystery. Little by little our knowledge has been extended, and Professor Oersted in 1819 proved, by inductive reasoning, that a conductor of electricity, while a current was flowing through it, had all the properties of a magnet. That was a grand and most useful discovery, for without the knowledge of the laws then opened up our present system of electric telegraphy could never have existed.

Professor Oersted's discovery is well illustrated in the principle of Sir Wm. Thomson's siphon recorder, as used for signalling through long submarine cables. Imagine the conductor of a cable terminating in a coil of fine wire, suspended like the float at the end of a fishing rod, but placed in a strong magnetic field instead of water. The light coil thus suspended becomes magnetic when the slightest current of electricity is passing through it, and is consequently deflected to the left or right, according to the direction of the current.

I will mention an experiment lately made by me, which I think favours Professor Hughes's observations on the molecular theory. If a flat spiral of copper wire be placed in circuit with a battery and suitable contact breaker, then any piece of iron placed in the neighbourhood of the spiral will emit a sound at each make or break of the battery. Recently I placed in front of a spiral so connected, a sheet of thin iron fastened to a sounding-board on which was placed a microphone. The microphone was then connected, as usual, to a battery and telephone, and on setting the contact breaker (an automatic tuning fork transmitter) to work, the pitch of the tuning fork was clearly reproduced in the telephone, the molecular vibrations of the iron plate being sufficient to affect the microphone.

I have often endeavoured to picture to my mind the sensation that must have been caused by Professor Oersted's discovery, but I think that what Professor Hughes has shown us this evening will be as equally exciting. I have never listened with more satisfaction to the reading of any paper than I have this evening; and I am sure that it is almost unnecessary for me to call upon you to give a hearty vote of thanks to Professor Hughes for his kindness in bringing before us the results of his investigations.

Professor W. GRYLLS ADAMS, F.R.S.: I entirely agree, Sir, with the remarks which have fallen from you as to the value of the paper we have just heard. It possesses one great merit, in that it puts before us very clearly the different theories of magnetism which have been propounded by Poisson, by Coulomb, by Ampère, by Weber, and by Wiedemann. The first part of the paper is a résumé of the different theories of magnetism, which Professor Hughes proceeds to clear away to make room for his new theory. The principal point of his theory is contained in his fourth statement, that "when we have external neutrality, or no apparent magnetism, the molecules or their polarities arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction." Thus he gets rid of the notion that the molecules of a magnetic substance have their magnetic axes pointing in any direction indifferently when the substance is not magnetised. This is a very important point in theories of magnetism, and Professor Hughes's experiments go a long way in support of his position, that in a state of neutrality the molecules take the position which they would freely take under the action of the earth alone. Some of the experiments which Professor Hughes has brought forward are quite explained by the previous theories as to the relation between currents and magnets, and are at the same time very pretty illustrations of the relation to the principles of energy.

In one experiment Professor Hughes showed us the effect of mechanical torsion on a wire through which a current of electricity was passing: when the wire was under torsion the current deflected a magnet which was placed at right angles to the wire. This result is entirely explained by the ordinary laws of the action of currents on magnets. When there is no torsion, the current is everywhere parallel to the length of the wire, but when the cylindrical wire is twisted, the generating lines of the cylinder take the form of a helix (say, a right-handed helix) and are pulled in the direction of their length: the result of this is that the electrical resistance is increased in the direction of this right-handed helix, and consequently the current flows in a left-handed helix, crossing the generating lines.

The lines of magnetic force of the current will no longer be at right angles to the length of the wire, and so the magnetic needle at right angles to it is deflected. This is very well illustrated by a pretty experiment first made in my laboratory, by Mr. Bottomley and myself, some years ago. A brass tube was firmly fixed at one end and placed at right angles to the magnetic meridian; a magnetic needle with a mirror attached, as in a reflecting galvanometer, was suspended within the tube; a scale and lamp were placed opposite the open end of the tube, to which torsion could be applied by means of two arms projecting from the end. On sending a current along the brass tube, the magnetic needle within it was not deflected, but on applying torsion to the tube whilst the current was still on, the needle was immediately deflected.

Professor W. E. AYRTON, F.R.S.: All of the experiments made by Professor Hughes have interested me very much, but that part which referred to the reaction of the magnet on the current had especial interest. In 1872 a somewhat similar idea struck me, viz., that if you were to act magnetically on a wire you would probably change the path of the current, and so alter the resistance of the wire. For that purpose (I was at Messrs. Hooper's works, Millwall, making certain tests on the Great Western cable at the time) I had constructed a coil of iron wire, by the side of which an insulated copper wire was placed, the object being to ascertain whether the resistance of the iron wire was not altered by being transversely magnetised. Some preliminary experiments showed that a certain change was produced in the ordinary electric resistance of an iron wire by keeping it in a constant state of transverse magnetism—that is to say, the facility for electricity to pass along the iron wire was altered by being magnetised. Having to leave at the beginning of 1873 for Japan, I was unable to complete those experiments, and therefore the numerical results were never published, although the fact, I believe, gained publicity. Since that time far more elaborate experiments have been carried on at King's College, and also by Beetz and by De Lucchi, and some very perceptible changes in the electric resistance of iron wires by magnetising them have been observed. Professor Hughes' very pretty experiment, showing the action on the current due to the molecular magnets composing the bar, of course gives additional confirmation to this idea.

I do not know that I quite understand the exact arrangement he imagines for the little molecular magnets when the bar does not show any trace of magnetism. Perhaps Professor Hughes will be so kind as to add a word or two on this point. I would ask him what is the exact arrangement he pictures in his mind's eye as being taken up by the molecular magnets when the bar is not magnetised? When, for example, you have a very thick iron room on which the effect of the earth would not be felt by the experimental inside bar, I would ask what he then thinks is the arrangement of the molecular magnets in an iron bar inside such a room?

Professor Hughes' experiments are certainly very novel and most interesting, but may, I venture to think, be explained in accordance with the older theories of magnetism. One of Professor Hughes' objections to Ampère's theory of magnetism is "that we have no knowledge of any elementary electric currents continually flowing without any expenditure of energy." But we all, I presume, admit that the molecules in any body are in a state of rapid vibration and possibly of rotation; consequently, if they possess static charges of electricity, the rotations of the molecules will cause these charges to act like currents, and without any expenditure of energy. I do not, of course, say that this is the explanation of magnetism, but I would point out that it is quite in conformity with Ampère's theory of magnetism.

Professor D. E. HUGHES, F.R.S.: In reply I would say that I quite agree with Professor Adams, who has very kindly explained the true mechanical operation which really takes place. I was afraid that time would not allow me to go into the explanation, but it is clearly shown in Wiedemann's book that every molecule is capable of turning in a spiral to 45°, when a rod is under the influence of torsion.

As regards Professor Ayrton's remarks regarding the mutual action of magnetism and electricity, I quite agree with them, as I have shown that the action and reaction on each other is equal. If a magnetic needle, or even a molecule, is perfectly free, then the current rotates the magnetic needle, but if the needle or molecule is rigid and cannot turn, then the electricity is deflected, producing the results I have shown this evening.

Professor Ayrton asks a very pertinent question regarding neutrality, but I believe he will find the answer to his question in my paper, where I have shown that we cannot obtain perfect neutrality in soft iron whilst under the influence of the earth's magnetism, but that we can produce a perfect external neutrality when the molecules form a closed circle of mutual attraction, as in circular or superposed magnetism.

If it were possible to place a piece of iron in a perfect neutral field, free from the earth's directing influence, then the molecules would react upon each other, through their mutual attraction, producing the closed circuit of mutual attraction, as I have already shown.

The **President**: The time has now arrived for us to close, and I have to remind members that a ballot will now take place, and then the meeting will adjourn to the 8th of November. The vacation appears a very long one, but I think that what we have heard this evening will provide sufficient entertainment to last us until we meet again.

At the conclusion of the ballot it was announced that the following were elected:—

As Foreign Members:

Eric Gerard.
Hermann Sedlaczek.
Julius Timm.

As Member:

Edward Tyer, F.R.A.S., F.R.G.S.

As Associates:

John Bailey, jun.
Leslie C. Bell.
L. Binet.
Colonel George E. Gouraud.
Henry Sutton.
Edwin C. Wallis.
Lieut. A. E. Wrottesley, R.E.

As Students:

Pedro Juan Gomez.
Alfred Edward Ruddock.

The meeting then adjourned until November.

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ACCESSIONS TO THE LIBRARY.

By Alfred J. Frost, *Librarian.*

(*Works marked with an asterisk * have been purchased.*)

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OBITUARY.

GENERAL SIR E. SABINE, K.C.B.

We regret to announce the death, on the 26th of June, of our honorary member General Sir Edward Sabine, K.C.B. The following memoir of him is taken from *Nature* of July 5, where it' follows the memoir of Mr. Spottiswoode, P.R.S., who died almost at the same time:—

"Spottiswoode, round whose grave in Westminster Abbey so many men, great in so many ways, have stood to-day, is not the only President of the Royal Society, and not the only man of science whose loss we have to deplore. While one, however, was cut off in the full tide of his life, and while there seemed to be a rich promise of many years of valuable work in store, the other had far outlived his working powers, and by many years exceeded those of his activity.

"A reference to the life-work of Sabine will clearly show how justly his high position and reputation were accorded to him, how nobly he has worked in the cause of science, and how imperishable a record of his life remains in the existence of a whole branch of scientific research, the foundation of which was mainly due to his untiring industry.

"Coming of an old family said to be of Italian origin, which early settled in Normandy, and removed thence to our own country, Edward Sabine was born in Dublin on October 14, 1788, being the son of Mr. Joseph Sabine of Tewin. He received his early education at the Royal Military Colleges of Marlow and Woolwich, obtaining a commission as second lieutenant when but fifteen years of age, and receiving his captaincy eleven years later.

"Very early in life, indeed, his interest became centred in physical science, and especially in magnetism, the study of which he pursued with indefatigable zeal and marked success. The result of his work in this and other fields is to be found in the *many* papers which issued from his pen. In 1818, six years before Spottiswoode was born, he was elected a member of the Royal Society, and in the same year was appointed astronomer to the expedition under the command of Sir John Ross which left England in search of the North-west Passage. The careful observations which he made whilst with the expedition were of great value. His published papers begin from this date, commencing with a contribution to the *Transactions of the Linnean Society*, on the birds of Greenland, the result of observations made during the voyage; they range from that date down to the year 1872, thus extending over a period of no less than fifty-four years.

"During this long period of active work he contributed to the *Transactions* and *Proceedings* of various societies and contemporary magazines upwards of one hundred papers, some of great length and many of considerable value and importance. Although a large number of these deal with the subject of terrestrial magnetism, many other branches of science are included in them, the voluminous nature of his published works being not less remarkable than the wide fields of study over which they range.

"A considerable number are to be found in the *Philosophical Transactions*, to which he contributed upwards of forty. To the *Proceedings of the Royal Society* he made numerous contributions during his long association with it; in the *Quarterly Journal of Science* he published twelve papers, in the *Reports of the British Association* we find ten, to the *Philosophical Magazine* he made eight contributions, the remainder of his published works being scattered among the *Edinburgh Journal of Science*, *Journal of he Geographical Society*, the *Proceedings* of one or two foreign ocieties, and the pages of foreign scientific magazines.

"As we have already said, his scientific contributions date from is voyage to the Arctic regions with Sir John Ross in 1818. ext year he again went to the Arctic regions, this time with an xpedition under the command of Sir Edward Parry. As the esult of his observations there, he made two communications to e Royal Society, published in the *Philosophical Transactions*, ealing, the one with the irregularities observed in the direction of the compass needle consequent upon the attraction of the iron of the ships, the other with the variations of the magnetic needle, and the intensity of the magnetic force during the voyage, and calling attention for the first time to the extreme importance of founding a widely extended series of observations of those strange magnetical disturbances, the origin of which is still mysterious. With this object in view he left England two years later on a long voyage in H.M.S. "Pheasant," making numerous observations and bringing many new facts to light. At the same time, at several equatorial stations on the coasts of Africa and America, he made observations with regard to the swinging of the pendulum, with the object of determining the true figure of the earth, publishing the results in the *Philosophical Transactions*. When on the American coast during this voyage he took up, amongst other subjects, the question of deep-sea temperatures, and in the *Philosophical Transactions* for 1823, he at that early period published a paper on the temperature at great depths in the Caribbean Sea, whilst in the same year his busy pen was giving an account of the barometrical measurement of the height of the Sugarloaf Mountain at Sierra Leone, and the Pico Ruivo in the Island of Madeira. Three years later he published in the *Quarterly Journal of Science* an account of the ocean currents met by H.M.S. "Pheasant" during the voyage from Sierra Leone to Bahia, and thence to New York, in which he records that the Amazon stream was crossed at a distance of 300 miles from the mouth of the river. In this year (1823) he proceeded on another voyage, going this time in H.M.S. "Griper" to Norway, Greenland, and Spitzbergen, to continue his magnetical observations, and to extend the series of pendulum experiments. Whilst at the latter place he again took up the question of barometrical measurement of heights, publishing in the *Philosophical Transactions* for 1824 a comparison of the method of measurement with the trigonometrical determinations. Then in the *Edinburgh Journal of Science* in 1825 he dealt with the presence of the Gulf Stream on the coasts of Europe as determined by his observations in the year 1822, and proceeded to discuss the question of depression over the region occupied by the Stream.

"In 1826 an account of his magnetical observations at Spitzbergen appeared in *Poggendorff's Annalen.*

"Continuing his pendulum swingings in 1827, he set about determining by direct observation the difference in the lengths of the seconds pendulum at Paris and London. The results of these experiments were published in a paper of some length which appeared in the *Philosophical Transactions* for 1828. At the same time he also experimented with the object of ascertaining the ratio of the magnetic forces acting on a needle horizontally suspended in London and in Paris. In 1829, in the *Philosophical Transactions*, he wrote on the reduction to a vacuum of the vibrations of an invariable pendulum; and in the *Quarterly Journal of Science* for the same year he gave an account of experiments concerning the force of the earth's magnetism, and on the then recent magnetical observations in Siberia of M. Hanstein. In the *Philosophical Transactions* for 1831 he describes some experiments made with the object of determining the length of the seconds pendulum at Greenwich.

"For many years from this date he worked mainly at that science on which he had most deeply set his mark, that of terrestrial magnetism. In 1835, in conjunction with Lloyd, Humphrey, and J. C. Ross, he contributed to the *Reports of the British Association* (of which he was an early and active member, filling the post of General Secretary for twenty-one years) an account of the terrestrial magnetic force in Ireland. In the following year he himself published in the *Reports* of that Association an account of the magnetic force in Scotland. As an indication of his range of subjects, we may here remark that at this time he published in *Froriep Notizen* a paper concerning the volcanoes of the Sandwich Islands. Then in 1837 we find him again contributing to the *British Association Reports*, this time a paper on magnetic intensity, dealing with the variations it exhibits at different parts of the earth's surface. He also wrote on the same subject two years later in *Froriep Notizen*, *L'Institut*, *Quetelet's Mathematical Correspondence.* In 1838 a memoir on the magnetic isoclinal and isodynamic lines in the British Isles appeared in the *British Association Reports*, being prepared from observations made by Professor H. Lloyd, J. Phillips, R. W. Fox, Capt. J. C. Ross, and the indefatigable Sabine. In 1840 he continued his papers on terrestrial magnetism in the *Philosophical Transactions*, now taking for his subject the consideration of lines of equal inclination and intensity in the Atlantic Ocean, and on lines of magnetic intensity between the Cape of Good Hope and Australia. He added to this series in the following year by contributing an account of the observations made by Captain Belcher on the west coast of America and adjacent islands, and the new determination of magnetic elements at Otaheite. Writing in 1838, Sabine had so conclusively demonstrated the importance of magnetical observations being made in every part of the globe, that Captain James Ross was sent with the "Erebus" and "Terror" to make a magnetical survey of the Antarctic regions. Sabine did not accompany the expedition, but had the results of the observations regularly sent him. In extension of the work of the magnetic observatory which he had established in England, and which was carried on entirely by his influence, Sabine had induced the authorities to promote the establishment of observatories in the Colonies. On the voyage out, therefore, not only were numerous observations made, but magnetical and meteorological observatories were founded at St. Helena, the Cape, and Van Diemen's Land, thus permitting a great increase in the number of possible observations and a consequent more rapid advance of the science which Sabine had so much at heart. These observatories—to our disgrace be it said, some have now been abolished—were placed under the superintendence of Sabine, and at this period a general magnetic survey of the globe was commenced by him under the direction of the Admiralty, although from what has gone before it is easy to see that the initiative of such a gigantic task had come from himself.

"In 1842 he yet further added to his contributions to terrestrial magnetism, publishing in the *Philosophical Transactions* an account of observations made during the voyage of the "Erebus" and "Terror" from England to the Cape, and from thence to Kerguelen Island. Then in 1843 he wrote concerning the extension of these observations from Kerguelen Island to Van Diemen's

Island, giving an account also of the various observations made in the Antarctic circle itself during the summer of 1840 and 1841, adding in the year following (1844) an account of the observations from June, 1841, to August, 1842, in the same region. In 1844 and 1845 he made contributions to the *British Association Reports* concerning the meteorology of Toronto and Bombay. During 1846 he again made contributions to meteorological literature, discussing the winter storms of the United States, and the cause of the mild winters which occur sometimes in our own country.

"With reference to the survey of the globe to which we have referred, we find him next giving an account of a magnetic survey of a considerable portion of the North American continent, and of the southern hemisphere between the meridian of 0° and 125° east, and parallels of – 20° and – 70°. In 1849, in another contribution, he gave a map of the magnetic declination for 1840 in the Atlantic Ocean, between the parallels of 60° N. and 60° S. latitude. In this year it was that Humboldt's *Cosmos*, for the author of which Sabine had a profound admiration, began to be issued in England, being translated by Mrs. Sabine, and edited by her husband, it being completed in 1858. In the year following he become vice-president of the Royal Society, with which he had been so long connected.

"The colonial observatories were, as we have said, under the control of Sabine, and remained so for many years. In 1851 and 1852, and again in 1856, he continued his papers on the magnetism of the earth.

"It had been observed (first by Lamont) that the mean of the larger magnetic disturbances gave signs of being bound by some law, and of having a definite but long-period variation. Previously to this it had been shown by Schwabe that the number of spots on the surface of the sun increased and decreased in obedience to regular law, the cycle occupying nearly eleven years for its completion. The results of the observations at the colonial observatories led Sabine to the discovery that magnetical disturbances were intimately bound up with this solar spot period; that the connection between them was of such a nature that a year of large declination coincided with a year of maximum sunspots, whereas those years when the range in declination was small corresponded with years when there were but few spots on the sun. In the same year the same fact was independently determined by Dr. Rudolf Wolf and M. Gautier.

"In 1853, at the meeting of the British Association at Belfast, Sabine occupied the presidential chair. In this year he turned to a consideration of the moon's influence on terrestrial magnetism, writing concerning the effect of that body on the magnetic declination at Toronto, St. Helena, and Hobarton; and taking up the subject again in 1856, he then discussed the lunar diurnal variation at Toronto. At a later period, in the *Proceedings of the Royal Society*, he contributed a paper on the lunar diurnal magnetic declination obtained from the Kew photograms. In 1857 he made another contribution to the *British Association Reports*, discussing the amount and frequency of the magnetic disturbances and of the aurora at Point Barrow, on the shores of the Polar Sea. In the *Philosophical Transactions* for the same year he discussed the question of the existence of the decennial period in the solar diurnal magnetic variations and its non-existence in the lunar diurnal variation of the declination at Hobarton, as M. Kreil seemed to think was the case. He then stated, as the result of a re-examination of the question by the light thrown upon it by the Hobarton observations, that he was as entirely convinced of the existence of this period in the former case as he was convinced of its non-existence in the latter.

"Continuing the investigation of this subject, he contributed to the *Royal Society Proceedings* for 1859-60 a paper on the solar diurnal variation of the declination at Pekin. In the same volume of the *Royal Society Proceedings* he also wrote concerning the laws of the phenomena of the larger disturbances of the magnetical declination at Kew Observatory. In 1861, at the request of the General Committee of the British Association, he prepared a report on the repetition of the magnetic survey of England. In this year he succeeded Sir Benjamin Brodie in the presidency of the Royal Society, which position he occupied for the next ten years. In the *Philosophical Magazine* for 1862 he entered into a discussion concerning the cosmical origin of terrestrial magnetism. Two years later, both in the *Philosophical Magazine* and the *Proceedings of the Royal Society*, he published a comparison of the most notable disturbances of the declination at Kew and Nertschinsk during 1858 and 1859. During the next few years, notably in 1866 and 1871, records of the magnetical observations at Kew were published by him. The chief work, however, of this period of his life consisted in concluding his contributions to the *Philosophical Transactions*, by reports and reductions of the work done during the Antarctic expedition. In a lengthy contribution in 1866 he resumed the discussion and co-ordination of the various observations, continuing and concluding this in another paper, which is to be found in the *Transactions* for 1868. His last contribution appeared in 1872, when he gave a magnetical survey of the North Polar regions to serve as a companion to the survey of the South Polar regions which had already appeared. It was his earnest wish that he might be spared to complete this, but the infirmities of age were then stealing over him, and it is doubtful whether it would ever have appeared had it not been for the able assistance of Captain, now Sir Frederick Evans, the Hydrographer of the Admiralty, assistance which the author gracefully acknowledges in a postscript to the memoir.

"From this date the work of Sabine may be said to have ceased. He had resigned the presidency of the Royal Society the previous year, and he now sought to spend the evening of his life in that retirement and rest to which his advanced age and great works so fairly gave him a claim. He had received the Copley Medal of the Royal Society in 1821, and the Royal Medal of the same society in 1849. In 1869 he was made K.C.B. He possessed also the Prussian order *pour le mérite*, and was either an honorary or corresponding member of many foreign societies. We mention these facts to show that he retired from his active life full of well-earned honours. In 1879 he lost his wife, who for more than half a century was the close companion of his labours. In the history of the Royal Society his name will ever be valued as that of one who, both as member and as President, was ever foremost in guarding its honour and maintaining its dignity, whilst the kindness and courtesy which as President he displayed to all, not excluding the younger members, will be always gratefully remembered.

"It is chiefly by his pendulum observations and by his magnetic determinations and reductions that, as may be gathered from what has been said, his name is so well known in science. The degree of accordance which some of the early determinations of the former kind exhibited was so much in advance of what was at that time thought likely, that they were received with incredulity in some quarters. The discussion which Sir George Airy made long ago, in his article on the figure on the earth, published in the "Encyclopædia Metropolitana," of the pendulum observations then available for that purpose, shows how large a share belonged to the labours of Sir Edward (then Captain) Sabine.

"His own magnetic observations were marked by his wonted accuracy; and his discussion of the results obtained at the colonial magnetic observatories led to new and unexpected results. The most striking, perhaps, of these was the discovery of the relation between magnetic perturbations and the more or less spotted condition of the sun's suface, to which we have already referred. Dissimilar as are these phenomena, and difficult as it then at least was to imagine any possible cause for a connection between them, subsequent observations have fully confirmed the conclusion at which he arrived, that connected they are, though what the precise nature of the connection may be is still a matter of discussion.

"Though from the nature of the case the work was one of compilation rather than of original observation, his determination of the magnetic state of the earth at a particular epoch, with its accompanying maps of the isoclinal, isogonal, and isodynamic lines was most noteworthy. The search for the original authorities and the application of the corrections requisite to render the observed results comparable with one another occupied a long time, and the results, as we have pointed out, appeared in instalments, as the various regions into which as a matter of convenience the earth's surface was divided were successively completed.

"The establishment of the colonial observatories, too, was the direct result of his exertions; and his name will go down to posterity as that of the man who more than any other laboured for the proper establishment of the science of terrestrial magnetism, interesting and important in its scientific aspect, and pregnant with so many benefits to mankind at large.

"He was buried on Saturday, his remains being placed beside those of his wife in the family vault at Tewin; the funeral, in accordance with his own wish, being of the simplest character. In addition to the members of his family and private friends, the funeral was attended by the Secretary and Treasurer of the Royal Society, the Hydrographer to the Admiralty, and representatives of the other Government services with which he had been so long connected."

ORIGINAL COMMUNICATIONS.

NOTES OF OBSERVATIONS OF SOME EARTH CURRENTS, APPROACHING AN ELECTRIC STORM, WHICH PREVAILED FROM THE 17TH TO THE 22ND NOVEMBER, 1882.

By T. Ishie,
Director-General of the Imperial Japanese Telegraphs, and the Society's Local Honorary Secretary for Japan.

Tokio, *April*, 1883.

17th.—Earth currents appeared first on the evening of the 17th November, causing interruptions simultaneously on all the lines, with the exception of a few short circuits. Continuous tests (taken with a tangent galvanometer at the Nagasaki Telegraph Office) on the main lines, some of which are not less than 500 English miles long, showed that the earth currents were prevalent on all these wires, the negative and positive waves coming on alternately in short durations. At 8.57 p.m. interruption of communication commenced, and lasted for about an hour, the maximum current observed in this interval being at 9 p.m. when it reached about $\frac{1}{30}$ part of an ampère; it then gradually decreased until midnight, when it almost entirely disappeared.

18th.—At 1 a.m. earth currents reappeared on the various lines, their strength increasing gradually until they reached a maximum at 8.30 a.m. The maximum intensity of the current observed in this interval was as great as that observed on the preceding evening. Interruption of communication continued for about thirty minutes.

19th, 20th.—From 9 a.m. of the same day until 7 p.m. of the 20th, earth currents were occasionally observed, but were not of such strength as to materially interfere with the working of the lines. During the interval between 8 p.m. and midnight, however, the working on all lines continued troublesome. For about twenty minutes (between 10 and 11 p.m.) communication on the longer lines was altogether stopped.

21st, 22nd.—The ordinary tests, taken on the morning of the 21st and 22nd, showed that the lines were not yet free from earth currents, which, though slight, were distinctly observable.

Comparing the above-mentioned observations with those published in the *Telegraphic Journal* of the 25th November, we see that the electric storm in Japan occurred nearly at the same time as those in Europe and America; hence it would appear that the electric storm of November last prevailed all over the world.

(*Extracted from Notes made by Mr. M. Yoshida, Electrician to the Japanese Telegraph Department.*)

[**Note.—The clock at Nagasaki is about 8 hours 39 minutes in advance of the clock at Greenwich.**]

BATTERY POLARISATION.

By A. Eden, Associate.

In the course of experiments undertaken with a view to lessen the effects of polarisation in galvanic batteries, the following arrangement was found to be advantageous, while admitting of approximately correct measurements of the opposing electro-motive force produced by the deposition of hydrogen on the negative plate of those cells which do not contain metallic solutions.

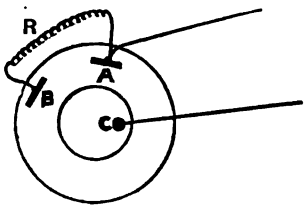
As I am not aware that the method referred to has been tried before, I trust that it will be found of sufficient interest to warrant my describing it to the members of this Society.

Any cell subject to polarisation is taken and furnished with a second negative plate of the same material as that previously in the cell, the two plates being free from contact in the liquid, but connected externally by a resistance, as shown in figure.

When the wires leading from A' and C are connected by a wire of (say) 40 ω resistance, the path C B R A is traversed by a very feeble current, while C A is the route taken by a current of much greater force, hydrogen being formed with more or less rapidity on A, while B is practically free from it.

As the current continues to flow, a galvanometer inserted in R indicates an increasing deflection (up to a certain maximum), while if the ordinary path, C A, is disconnected, the deflection in R gradually declines and ultimately ceases, indicating that A is no longer positive to B, or, in other words, that the hydrogen has been converted into electricity, and subsequently into heat or work.

It is obvious that, if the circuit A B R did not exist, the hydrogen would not be so expended, but would be dissipated very slowly.



The circles in the above sketch represent the outer and the porous pot respectively.
A, the usual negative plate.
B, an additional negative plate of the same material.
C, the positive rod or plate.
R, a resistance coil of 100 ω or more.

To illustrate this, I give the mean results of several tests made with a Fuller's bichromate cell, pint size—internal resistance, 1·5 ω; E.M.F., 2·03 volts :—

	E.M.F.
1. Hydrogen circuit disconnected; battery short-circuited 1 minute	1·88
2. Force indicated in polarisation circuit when joined up at moment of disconnecting battery in No. 1... ...	·15
3. As No. 1 for 5 minutes	1·74
4. Force indicated as No. 2 after disconnecting in No. 3	·29

	Hydrogen circuit disconnected. Volts.	Hydrogen circuit joined up. Volts.
5. After 5 minutes short-circuiting ...	1·74	1·82
6. Battery working through 1,070 ω after short-circuiting rose from 1·74 to 1·96 in	5 min.	2 min. (See No. 8)
and to full force in	(Would not rise above 1·96)	2 m. 50 s.
7. After 5 minutes short-circuiting, battery disconnected, and rose to full force in	6 min.	2 m. 50 s.
8. Hydrogen circuit taken away; two negative plates joined together so as to act as one plate, short-circuited for 3 minutes	1·82 volts.	
9. As No. 8, then disconnected, but did not rise from 1·82 to full force for ...	9 m. 15 s.	
10. As No. 8; did not fall to 1·74 until end of 10 minutes, but took 10 minutes to rise to full force after being disconnected.		

Experimenting with different solutions and also different carbons, it appeared that—

1. The density of the plates influenced the rapidity of polarisation and depolarisation.

2. Polarisation and depolarisation seemed to occupy nearly equal periods when the polarisation circuit was not closed.

3. When the polarisation circuit was in action, polarisation was effected with much greater difficulty, and depolarisation took less than half the usual time.

4. The resistance of the hydrogen was not appreciable.

Practically, it would seem to be advisable to put two negative plates (arranged as described) in each cell liable to polarisation; and it seems conclusively proved that a single increase of negative surface, by placing the negative plate in fragments of the same material, or by using larger plates, tends to retard the fall of force produced by polarisation, but also retards recovery in the same degree.

EDINBURGH, 11*th January*, 1883.

ABSTRACTS.

E. WARBURG—ON EFFECTS OF RETENTIVENESS IN THE MAGNETISATION OF IRON AND STEEL.

(*Phil. Mag., Vol. XV., No. 94, April, 1883, p. 246.*)

The author refers to a note of Mr. Ewing, communicated to the Royal Society, in which is described a fundamental fact previously published by the author. If a permanent moment, m_0, has been produced in an iron wire by the action of a longitudinal magnetising force, K_1, and if the wire be then subjected to magnetising forces which first increase continuously from 0 to K_1, and then decrease continuously from K_1 to 0, then for the same magnetising force, K, the magnetic moment of the wire will be found greater when K is increasing than when it is decreasing. After some repetitions of the operation the wire will be found in a stationary condition, in which for $K = 0$ it always has the same moment, m_0, and for $K = K_1$ the same moment, $m_0 + m_1$. If, therefore, the wire being in this state, its moment is represented graphically as a function of the magnetising force, K, a closed curve will be got. Except the minimum (0) and maximum (K_1) values of K, two values of m correspond to every value of K.

R. H. M. BOSANQUET—ON PERMANENT MAGNETISM.

(*Phil. Mag., Vol. XV., No. 95, May, 1883.*)

In this original communication the author considers some experiments which he has been making on the subject. He takes the view that magnetic induction, or the number of lines of force through unit area, is the quotient of magneto-motive force by resistance in magnetic circuits. Magneto-motive force is a difference of magnetic potential. The author has shown in a previous paper that if a permanent magnet of hard steel be divided into short lengths, the sum of the moments is not the same as before, but is greatly reduced. The experiments were made with a compound magnet, constructed of eighteen cylindrical pieces fitted, hardened, and magnetised. They were placed in a cradle hung in a bifilar suspension arranged so that the plane of zero deflection was at right angles to the magnetic meridian. Then for weeks the following mean deflections were obtained:—

For the eighteen pieces joined up	13·1°
For the eighteen pieces separated...	1·8°

A. L. KIMBALL—NOTES ON THE DETERMINATION OF THE OHM IN ABSOLUTE MEASURE.

(*A Communication to the University Scientific Association, Jan.* 3, 1883.)

Notice was given of the proposed redetermination of the ohm; the method to be used being the same as that used by Prof. Rowland in 1876, changes being made in the character and arrangement of the apparatus, so as to avoid, so far as possible, the repetition of constant errors. A short account was given of the nature and importance of absolute measurement in general, in which the derived units are all based on the fundamental units of length, mass, and time. The nature of the unit of electrical resistance was then noticed, and attention called to the fact, that in the electro-magnetic system of units the unit of resistance bears to the units of length and time the relation of a velocity. Mention was made of the most noteworthy methods that have been used in determining the value of resistance in absolute measure, attention being called to the manner in which the units of length and time entered into the experiments.

REPORT ON THE DYNAMO-ELECTRIC MACHINES USED FOR THE TRANSMISSION OF POWER BY M. MARCEL DEPREZ. BY MM. BERTRAND, TRESCA, DE LESSEPS, DE FREYCINET, AND CORNU.

(*La Lumière Électrique, T. VIII., No.* 15, *Avril* 14, 1883.)

This is a reproduction of the official report presented to the Academy of Science of Paris, by the Commission appointed to carry out the experiments with the above machines. These experiments were made the 4th March last, in the workshops of the Northern Railway of France; and the results have already been published in full. (*Vide The Electrician*, March 31 and April 7, etc.)

DE NERVILLE—NEW EXPERIMENTS OF M. BJERKNES.

(*La Lumière Électrique, T. VIII., No.* 15, *Avril* 14, 1883, *p.* 479.)

The former experiments of M. Bjerknes simulated electro-magnetic or electrostatic phenomena; the present ones are the analogues of electro-dynamic phenomena. It may be as well to remark that the hydro-dynamic phenomena studied by M. Bjerknes are exactly opposite in their results to the corresponding electrical ones; thus, for example, two oscillating spheres attract one another when they are in the same phase, and repel one another when in unlike phase.

By using hollow bodies of various forms, which are caused to pulsate in a slightly viscous medium, such as glycerine or syrup, by means of rapid puffs of air sent into them from a double-action pump, the reproduction of various electro-dynamic phenomena can be effected. The hollow bodies used consisted of small drums, or cylinders, closed by membranes of india-rubber: these cylinders give rise to a field of force, in which the lines of force are circular, concentric, and perpendicular to the direction of the current, as would be the case with a straight wire conveying a current.

M. Bjerknes has investigated three cases:—

1. Study of the hydro-dynamic field of a revolving cylinder; corresponding to the electro-dynamic field of a current.

2. Mutual action of revolving cylinders; corresponding to the mutual action of currents.

3. Action of revolving cylinders on pulsating and oscillating bodies; corresponding to the action of currents on magnetic poles or magnets.

The figures obtained correspond exactly with those got by exposing iron filings to the action of a current; and from the first series of experiments it was found that a cylinder in rotary oscillation corresponds to an electric current, and that if two such cylinders are compared, they correspond to two currents in the same direction if their oscillations are concordant, but that if their oscillations are discordant they represent two currents in opposite directions.

M. Bjerknes has exactly reproduced Ampère's parallelogram by employing four cylinders forming the sides of a small square, and working into each other by means of bevel gearing: the cylinders are set in motion as before by means of an air-pump. All the phenomona of attraction, repulsion, and rotation can be reproduced with this apparatus.

To represent the action of currents on magnets a rotary oscillator is used, together with a sphere oscillating on a vertical stalk. From the former experiments it was found that such an oscillating sphere behaved like a magnet when in presence of a similar sphere, and the later experiments have shown that it still represents a magnet. Thus, if the cylinder and the sphere are put in oscillation, the latter turns round and takes up such a position that its oscillations are perpendicular to the direction of the cylinder.

M. Bjerknes is still prosecuting his researches, and hopes soon to find still further analogies between electro-dynamic and hydro-dynamic phenomena.

E. HOSPITALIER.—EXPERIMENTS WITH TROUVÉ'S NEW BICHROMATE BATTERY.

(*L'Électricien, T. V., No.* 50, *May* 1, 1883, *p.* 404.)

Each battery consists of six cells of ebonite of rectangular form; the whole placed in a wooden case, with an arrangement for raising and lowering all the plates at once. Each cell has one plate of zinc and two of carbon, the latter being coppered at the top, for the purpose of insuring good connection.

The solution for six cells is made of

Water	8·0	kilogrammes.
Powdered bichromate of potash	1·2	„
Sulphuric acid	3·6	„
	12·8	„

The solution contains, therefore, 150 grammes of bichromate per litre; but this may be increased to 200, or even 250 grammes per litre, if necessary. The solution is best prepared by first putting 150 grammes of bichromate in powder into a litre of water; after thoroughly stirring, the acid is *very* slowly added in the proportion of 450 grammes per litre of water, continuing to stir; the liquid, which has become hot by the addition of the acid, is then allowed to cool gradually. The total weight of a battery of six cells is about 33½ kilogrammes (say, 74 lbs.).

Twelve cells were connected in series, and used to work Swan lamps, all parallel. The plates were gradually lowered into the liquid so as to maintain a constant current of 8 ampères during four hours and a quarter, by the end of which time the plates had been completely immersed. During a further hour and 25 minutes the current gradually and regularly fell to 5 ampères, when the experiment was stopped.

During the former period of 4¼ hours—

Difference of potential at lamp terminals = 14·15 volts.
" " battery " = 16·70 "
Current ... ... = 8 ampères.
Work in external circuit *per second* ... = 133·6 watts.
" " " ... = 13·5 kilogrammètres.
Time ... ... = 15,300 seconds.
Quantity of electricity ... ... = 122,400 coulombs.
Energy available in external circuit = 206,550 kilogrammètres.

During the second period of 1 hour 25 min. the current had a mean value of 6·55 ampères during 5,100 seconds, and the mean electrical energy available in the external circuit was 9 kilogrammètres per second. The total energy available was therefore 253,350 kilogrammètres, or nearly one horse-power (= 270,000 kilogrammètres); thus one battery of 6 cells can furnish about half a horse-power. The material used up was—

Zinc	1,493 grammes.
Bichromate of potash	2,400 "
Sulphuric acid	7,200 "

MARCEL BRILLOUIN—METHODS FOR THE DETERMINATION OF THE OHM.

(*Journal de Physique, T. II., No. 16, Avril, 1883, pp. 149-158.*)

In the electro-magnetic system of units the ratio of a coefficient of induction to a resistance is a time. The only measurements essential for the absolute determination of a resistance are therefore—

1. The measurements of length necessary for calculating the absolute value of a coefficient of mutual induction.

2. A measurement of time.

The author proposes to employ, as a source of electricity, a magnet rotating inside a fixed spherical bobbin. The speed of rotation of the magnet may be high and very constant, and it can be accurately determined. The ends of the wire of the fixed bobbin are joined to a complex circuit, in which are induced currents of different amplitudes and phases, but all lasting during one revolution of the magnet. As the currents are undulating and alternating, it is necessary to use an electro-dynamometer as measuring instrument; and the circuit must be so arranged as to reduce the deflection of the movable bobbin to zero.

Two methods may be employed. The first method is shown in the following diagram:—

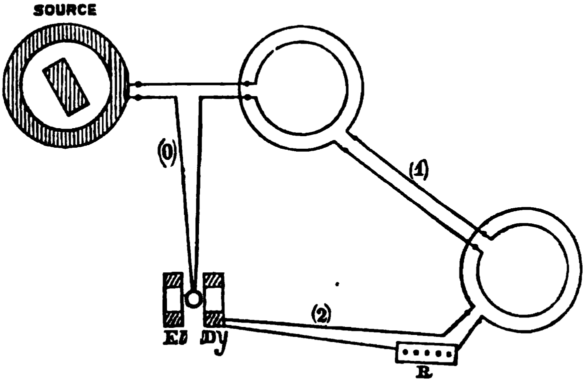


Fig. 1.

The conducting wires are arranged to form three distinct circuits, (0), (1), (2).

The circuit (0) comprises the undulatory source of the current, the movable coil of the electro-dynamometer, and the primary of an induction coil.

The circuit (1), traversed by an induced current of the first order, contains the secondary of the induction coil in the circuit (0), and the primary of a second induction coil, the secondary of which is in the circuit (2), comprising also a set of resistances and the stationary coil of the electro-dynamometer.

When the speed of rotation is sufficiently high, it is possible, by means of the box of resistances, to bring the electro-dynamometer to zero by making the difference of phase between the primary current and the induced current of second order equal to $\pi/2$.

Let R_1, R_2, L_1, L_2 be the resistances and coefficients of self-induction of the circuits (1) and (2), and M_{01}, M_{12}, M_{02} the coefficients of mutual induction of the circuits (0, 1), (1, 2), (0, 2), while K is the speed and θ is the very small error possible in the difference of phase $\pi/2$; then in each experiment M_{02} and θ are to be reduced to the lowest possible value, and the calculation made, treating these two quantities as nil, from the equation

$$\frac{L_1 L_2 - M_{12}^2}{R_1 R_2} K^2 = 1 .$$

The above equation may be used in two ways:—

1. By determining L_1, L_2, by comparison with M_{12}, which is calculated, a determination which cannot involve an error greater than $\frac{1}{1000}$;

2. By making two experiments with the same speed, in which L_1, L_2, R_2 remain the same, but R_1 is increased by r, and M_{12} changed into m_{12}.

We shall then have

$$\frac{L_1 L_2 - m_{12}^2}{(R_1 + r)R_2} K^2 = 1$$

which, combined with the previous equation, gives

$$(M_{12}^2 - m_{12}^2) K^2 = rR_2,$$

in which the coefficients of induction, M_{12} and m_{12}, can be calculated directly. In either method, R_2 will be obtained in terms of R_1 or of r, without appreciable error. The speed being measured in seconds, the experiments will give the absolute value of R_1 or of r.

In the second plan a differential electro-dynamometer is used, and the apparatus is arranged as shown in Fig. 2.

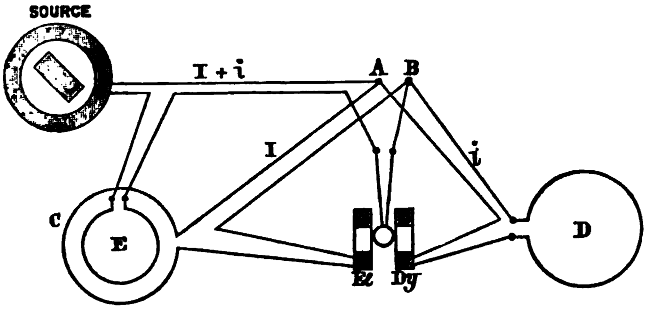


FIG. 2.

Between the points of derivation A, B and the electro-dynamometer are placed the resistances and two identical coils C, D, one of which, C, is the secondary of an induction coil, of which the primary, E, is directly in circuit with the source.

If I and i are the currents in the derived circuits A C G B and A D G B respectively, the mean deflection of the differential electro-dynamometer can only be reduced to zero for equal amplitudes of the two currents I and i. By means of the resistances this can be effected for a certain speed.

Let R, r, L, l be the resistances and coefficients of self-induction of the two circuits A C G B, A D G B, and M, m the coefficients of induction of each of them with reference to the circuit B S G A of the source, while N is their coefficient of mutual induction, then we arrive at the equation

$$R^2 - r^2 = \frac{4\pi^2}{T^2}(L + l - 2N)(l + 2m - 2M - L).$$

This equation may be used in several ways, but the author prefers the following:—In a preliminary experiment the two terms of the equation above are separately reduced to zero. The spherical bobbin is then replaced by a battery, and an independent current is passed through the movable coil of the electro-dynamometer. The resistances and the coefficients of induction are so arranged that the instrument shows no deflection; then

$$R = r;\ L + 2M = l + 2m.$$

The second equation is satisfied if the two derived circuits comprise the similar coils C, D, the coil E being removed.

This done, the connections are replaced as in Fig. 2, and E is brought up to C; M increases by M_0. A fixed resistance, R_0, is added to A D B, and the speed of rotation of the magnet is adjusted so that the electro-dynamometer shows no deflection. We shall then have

$$R^2 - (R_0 + R)^2 = \frac{4\pi^2}{T^2}(L + l - 2N) 2 M_0,$$

and the experiments will give R_0 in terms of M_0 and T.

Instead of proceeding by a method of comparison, three separate experiments may be made, with the same speed of the magnet, with three different sets of values R, R′, R″, and L, L′, and L″, when the three corresponding equations reduce to

$$\frac{R'^2 - R^2 + \frac{4\pi^2}{T^2}(L' - L)^2}{L' - L} = \frac{R''^2 - R^2 + \frac{4\pi^2}{T^2}(L'' - L)^2}{L'' - L}.$$

The author concludes by drawing attention to the fact that his is a null method, and therefore capable of great precision, and proposes in a second paper to give some particulars of the experiments now in progress.

A. POTIER—THE EFFICIENCY OF A SYSTEM OF TWO DYNAMO MACHINES.

(*Journal de Physique, T. II., No.* 16, *Avril*, 1883, *pp.* 172, 173.)

The author refers to the well-known formulæ,

$$T = \frac{E(E - e)}{R}, t = \frac{e(E - e)}{R}, I = \frac{E - e}{R},$$

which, when the current traversing a system of two dynamos can be considered constant, determine the power (T) put into, and the power (t) taken out of the system. The ratio $^t/_T = {}^e/_E = K$ is the theoretical efficiency of the system—an efficiency which in practice differs from the real efficiency the more the speeds increase. Keeping to this theoretical efficiency, and replacing t by K T in the above formulæ, we have

$$e^2 = R\,t\,\frac{K}{1 - K};\ E^2 = R\,t\,\frac{1}{K(1 - K)},$$

from which it follows that if the power to be transmitted and the efficiency are given, the electro-motive forces of the two machines can be determined, and are proportional to the square root of the total resistance of the circuit and of the power transmitted. If, for example, $t = 3{\cdot}6$ H.P., $R = 300$ ohms, taking $g = 10$ as an approximation, then

$$e = 900\sqrt{\frac{K}{1 - K}} \text{ volts, and } E = 900\sqrt{\frac{1}{K(1 - K)}} \text{ volts,}$$

say, 900 volts and 1,800 volts; if an efficiency of 0·5 is enough, or 1,800 volts and 2,250 volts for the theoretical efficiency of 0·8.

If practical considerations limit the electro-motive force which can be used, then only the product R t must not exceed the limit $\frac{1}{4}E^2$; for example, if E must not exceed 2,000 volts and R is still 300 ohms, t would not exceed 4·44 H.P., and the theoretical efficiency, $\frac{1}{2}$, would be all that could be obtained

G. POLONI—PERMANENT MAGNETISM OF STEEL AT VARYING TEMPERATURES.

(*Journal de Physique, T. II., No. 16, Avril,* 1883, *pp.* 180-182. *Taken from Reale Accademia dei Luicei,* 1882.)

The method of research was that which has been employed by Van Rees, and consists in placing the bar to be experimented upon in a solenoid, and measuring the current induced in it when the bar is suddenly withdrawn to a practically infinite distance.

The bar was heated by means of a circular oil-bath, and rested on a plate which could be raised or lowered so as to bring more or less of the bar inside the solenoid, which was fixed.

The bar is rapidly withdrawn by means of cords and pullies, and the value of the induced current gives the quantity $\int_0^x m\,.\,dx$, in which dx is an infinitesimal length of the bar, m is the quantity of magnetism in the corresponding section, and x is the distance from the solenoid to the lower end of the bar in its initial position.

Assuming that m may be expressed by Biot's formula,

$$m = a\left\{\mathrm{K}^{-x} - \mathrm{K}^{-(l-x)}\right\}$$

where l is the length of the bar and a and k are constants, we shall have

$$\mathrm{M} = \int_0^x m\,.\,dx = \mathrm{A}\left\{1 + \mathrm{K}^{-l} - \mathrm{K}^{-x} - \mathrm{K}^{-(l-x)}\right\}$$

$$\text{where } \mathrm{A} = \frac{a}{\log.\mathrm{K}}.$$

The experiments agree very well with the formula, and the author has found—

1. The general course of the phenomena is similar for all the bars experimented on. Slight variations in the magnetism occur up to 180°, they then rapidly increase, attaining a maximum at about 190°.

2. The distribution of the magnetism for temperatures from 0° to 180° is very well represented by the formula given above. A diminishes with increasing temperature, while K remains practically constant. Above 180°, however, the phenomena are somewhat anomalous. Two bars of steel, which had been heated and cooled several times under the influence of the earth's magnetism before being magnetised, obeyed the same law at all temperatures: the neutral line always remained at the middle of the bar. On the other hand, with four bars magnetised before being exposed to any heat, and then heated with the north pole downwards, the neutral line approached the north pole when 1808 were exceeded, the displacement amounting to as much as ⅓ of the bar; but K remained generally constant. If the bars are then heated with the south pole downwards, the neutral line still approaches the north pole, and K varies in value from one temperature to another.

3. So long as the temperature of 180° is not exceeded, the values of M

corresponding to the normal state for each temperature are represented by a function of the form

$$M = M_0 + a\,t + b\,t^2.$$

For the normal conditions corresponding to the temperature 300°, two formulæ must be used,

$$M = M_0 - t\,(a + b\,c^t)$$
$$M^1 = M^1_0 + (300 - t)\ \left\{a^1 + b^1\,c^{(300 - t)}\right\},$$

the former from 0° to 190°, and the second from 190° to 300°.

The author has supposed that the variation in the magnetism due to the temperature was proportional to the variation in the conductivity of the steel. Thus the ratios of the normal magnetism and of the conductivity at 20° to the same quantities at higher temperatures are—

		100°	150°	180°	200°	300°
Magnetism	...	1·08	1·11	1·15	2·00	2·69
Conductivity	...	1·45	1·73	1·90	2·01	2·57

JOUBERT—NOTE ON THE THEORY OF ELECTRO-MAGNETIC MACHINES.

(*Comptes Rendus, T.* 96, *No.* 10, *March* 5, 1883, *pp.* 641, 642.)

The experiments made by M. Tresca with the machines of M. Deprez have for the first time given the value of the loss of power in the machine, other than that accounted for by Joule's law.

All continuous current machines are made up of a certain number of elements, such as the convolutions in the Gramme ring, which, when the machine is used as a motor, pass from a position where the potential energy has a maximum value, W_o, to an opposite position of minimum value, W_1. The difference, $W_o - W_1$, represents the electro-magnetic work done by the convolution in passing from the first position to the second. In order that the motion may be continuous, the direction of the current in the convolution must be reversed, *i.e.*, the electrical energy which it possesses must be destroyed, and the original energy, W_o, must be restored to it. This operation occurs twice for each convolution in every revolution. Leaving out the work done in the resistance, the return is therefore $\frac{W_o - W_1}{W_o}$

It is easy to calculate the lower limit of the electrical energy thus lost at each half revolution, and which, at least in part, shows itself in the form of sparks. If I_1 stands for the current-strength at the moment when it is destroyed in the winding, and l is the coefficient of self-induction of the winding, the loss will be equal to $\frac{l\,I_1^2}{2}$. If the ring is made up of p convolutions, and if it makes n revolutions per second, the loss per unit of time will be as a minimum value, $n\,p\,l\,I_1^2$ or $n\,L\,I_1^2$, if L is the coefficient of self-induction of the whole ring.

MARCEL DEPREZ—NEW EQUATIONS IN CONNECTION WITH THE TRANSMISSION OF POWER.

(*Comptes Rendus*, *T.* 96, *No.* 12, *March* 19, 1883, *pp.* 777-779.)

The author was desirous of expressing the laws of the electrical transmission of power by means of equations which should contain only mechanical quantities. These equations are, however, only true on the hypothesis that the machines are mechanically and electrically perfect,—that is to say, that they are free from friction and irregularities,—and that they are made up of an infinitely great number of infinitely small sections, whilst the helix contains no masses of metal which could give rise to Foucault currents.

On this hypothesis, let

F_0 be the tangential force (in kilogrammes) applied to the generator at a distance $\frac{1}{2}\pi$ from the axis (which corresponds to a circumference of one metre);

V_0, the velocity in metres per second of the point of application of the force;

f_0, a coefficient depending on the construction of the machine;

F_1, V_1, f_1, the corresponding quantities for the motor;

I_1, the current strength;

R_1, the total resistance of the circuit, including both machines.

The magnetic fields of both machines being saturated, we shall have

$$F_1 = f_1 I; \quad F_0 = f_0 I,$$

and since

$$I = \frac{F_1}{f_1}$$

$$F_0 = \frac{f_0}{f_1} \cdot F_1$$

The mechanical work absorbed in unit of time by the generator is equal to $F_0 V_0$, or to $V_0 \frac{f_0}{f_1} F_1$.

The work lost in the circuit in the form of heat is equal to $\frac{R I^2}{g}$ or to $\frac{R}{g}\left(\frac{F_1}{f_1}\right)^2$, and has to be deducted from the work absorbed by the generator, and there then remains for the useful work given out by the motor,

$$F_1 V_0 \frac{f_0}{f_1} - \frac{R}{g}\left(\frac{F_1}{f_1}\right)^2$$

Finally, the return will be

$$\frac{F_1 V_0 \frac{f_0}{f_1} - \frac{R}{g}\left(\frac{F_1}{f_1}\right)^2}{F_1 V_0 \frac{f_0}{f_1}} = 1 - \frac{R F_1}{g V_0 f_0 f_1}.$$

In the above equations the work absorbed, the work given out, and the return, all are expressed as functions of the speed of the generator (V_0) and of the load on the motor (F_1); the only electrical quantity being the total resistance of the circuit (R).

J. MOSER.—THE TRANSMISSION OF POWER BY ELECTRICAL APPARATUS ARRANGED AS BATTERIES.

(*Comptes Rendus, T.* 96, *No.* 10, *March* 19, 1883, *pp.* 779-782.)

The author proposes to apply the same method to the transmission of power as he has done to the transmission of sound by telephones, by arranging the sending and receiving apparatus partly in series and partly parallel. With a given conductor, if the current traversing it remains constant, the loss of energy in the conductor will also remain constant. If the loss in the apparatus can also be kept constant, then we have a constant total loss; and by increasing the work given out by the motors, the ratio of the energy lost to the work reclaimed will become less and less.

The two conditions—1st, constant loss in the conductor; 2nd, constant loss in the apparatus—may be realised by transmitting the energy of one electric battery to another. As an example, consider the question of accumulators. Let a constant current, I, traverse one accumulator. The total loss, $\frac{I^2 R}{g} = P \frac{\text{kg.m.}}{\text{sec.}}$, is made up of the loss in the conductor and the loss in the apparatus. Suppose the counter E.M.F. to be e volts, the useful work will be $\frac{eI}{g} = V \frac{\text{kg.m.}}{\text{sec.}}$, and the return will be $\frac{V}{V+P}$. With four accumulators arranged two in series and two parallel, we double the E.M.F. without changing the resistance. Consequently the current, I, being always supposed the same, the useful work will be doubled.

The author proposes then to work with dynamo machines in a similar manner, using a high difference of potential, by placing several machines in series, and getting rid of the increased resistance of the machines so arranged by placing others parallel.

J. VIOLLE—THE RADIATION OF SILVER AT THE MOMENT OF ITS SOLIDIFICATION.

(*Comptes Rendus, T.* 96, *No.* 15, *April* 9, 1883, *pp.* 1033-1035.)

At the International Congress of Electricians in 1881, the author proposed the radiation emitted by one square centimètre of melting platinum as a standard for photometric measures, and he has since made some experiments on the subject, using, however, silver in place of platinum.

A vessel of molten silver was placed under a thermopile, joined up to a mirror galvanometer. The radiations from the surface of the silver fell perpendicularly on the face of the thermopile through a hole one square centimètre in size, made in a hollow screen through which circulated a current of cold water. On allowing the silver, heated above its melting point, to cool down, the following observations were made:—

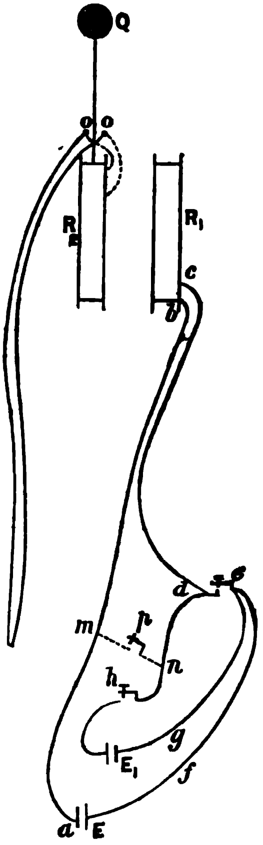
The radiation at first decreases more or less rapidly according to the shape of the containing vessel; this decrease then becomes less rapid, and at the instant of solidification at the sides of the vessel a slight increase occurs. As the solidification advances from the edges to the centre, an increase occurs, soon followed by a rapid decrease. The course of the phenomenon is essentially the same, whether the metal cools slowly or rapidly. The curve plotted from the readings of the galvanometer shows a portion which is very nearly a straight line, preceded and followed by a slight rise in the curve. This constant portion is clearly defined, and the author, therefore, is of opinion that in silver a fixed secondary standard might be found.

J. FRÖLICH—DETERMINATION OF THE OHM BY AN ELECTRO-DYNAMIC METHOD.

(*Annalen der Physik und Chemie, B. XIX., H.* 1, *No.* 5, 1883, *pp.* 106-130.)

Up to the present, all methods for the determination of the ohm which have led to fairly accurate results depended on the measurement of induced currents in closed conductors. Moreover, the measures in all cases were galvanometric: thus Kirchhoff, Rowland, and W. Weber measured the repulsive action of the induced current on a galvanometer needle. W. Weber also used the damping of the swing of a magnet by an induced current; and the British Association Committee and Lorenz dealt with the deflection of a needle by a constant current.

The author investigates mathematically the possibility of determining the standard of resistance also, by means of the electro-dynamic action of the inducing and induced currents. This part of the paper is unsuited for an abstract, but some of the actual observations may find a place. The diagram shows the general arrangement of the apparatus.



R_1 is the fixed coil, and R_2 the movable one. If *d* is joined to *e*, by closing the key at *e* we shall have a continuous closed circuit, *a m b c d e f a*, the resistance of which is r, and its coefficient of induction L. Let the stationary current be $I_1 = \frac{E}{r}$. Now close the key at *h*, then there is a second circuit, *d e g h n d*, with a resistance r_1, the part *d e*, common to the two currents, being negligible. Now adjust r_1 with respect to E_1 so that $I_1 = \frac{E}{r} = \frac{E_1}{r_1}$, which can always be done. Now break contact at *e*, and we shall have a closed circuit, *a m b c d n h g e f a*, with a resistance $r + r_1$, in which the E.M.F. $E + E_1$ acts, and in which the current will be

$$I = \frac{E + E_1}{r + r_1} = I_1,$$

therefore the same as before.

Now choose two points, *m n*, such that the resistance of the portion *m b c d n* equals *r*, and join them by a conductor, *m p n*, of negligible resistance. Then in the circuit *m b c d n p m*, at the moment of cutting out the E.M.F.'s $E + E_1$, the current I_1 is produced; its resistance is *r*, and its coefficient of induction equal to L.

The course of an observation will therefore be—

1. Make contact with the key *e*. An induced current is set up.
2. Make contact with key *h*.
3. Release key *e*. No spark.
4. Join *m* and *n* by key *p*. Induced current is set up.
5. Release key *h*. A spark is produced, but outside the circuit *m b c d n p m*.
6. Release key *p*. No spark, no current. Then repeat the above steps.

In the actual experiments the two coils R_1 and R_2 were similar; the coil R_2 was fixed to a horizontal arm, and attached to the bifilar suspension in the points *o o*, being counterbalanced by the weight Q. The plane of the coil was at right angles to the magnetic meridian, and the plane of the suspension wires, which were of copper, was in the magnetic meridian. The coil R_1 was fixed parallel to and concentric with the movable coil R_2. Each coil had 36 layers of insulated copper wire 0·33 mm. thick, and had respectively 1,056 and 1,058 convolutions; the resistance of each was 115 ohms. The mean diameter (R) of each coil was 5·2 cm. The distance of the axis of rotation from the centre of the movable coil (*l*) was 6·8 cm., the time of oscillation (T) 15·08 seconds, and its moment of inertia (K) 48,600 g. (cm.)^2.

These values substituted in the equation

$$\Phi = \frac{T}{\pi K} \cdot M \; \frac{\delta M}{\delta \phi} \cdot \frac{I_1^2}{r}$$

$$\text{give } \Phi = 3{\cdot}267 \; \frac{(\text{sec.})^2}{(\text{g.})(\text{cm.})} \cdot I_1^2 \, (\text{ampère}) \cdot \frac{1}{100}.$$

The experiments made were merely to see if the method were practicable, and the author, being satisfied on this point, proposes to carry out an extended series of observations.

F. KOHLRAUSCH—MEASUREMENT OF DIFFERENCES IN THE EARTH'S HORIZONTAL FORCE AT DIFFERENT PLACES.

(*Annalen der Physik und Chemie, B. XIX., H.* 1, *No.* 5, 1883, *pp.* 130-42.)

Observations by means of the oscillations of a magnet are open to the objection that they take a long time to make, and that in order to obtain a result within $\frac{1}{1000}$ the ratio of the oscillations must be correct within $\frac{1}{2000}$. Moreover, it is difficult to eliminate the effects of change of temperature with the large needles which must be used.

The author considers the method of deflections as better suited to the purpose, and he has proposed a modified instrument which he calls a "universal magnetometer," since it can be used as an ordinary magnetometer as well as for measuring variations in the earth's force at the same place at different times and variations in different places. The instrument has been described previously (*Annalen* 15, p. 545, 1882) and consists of a pillar on three feet with levelling screws, which carries a horizontal divided circle furnished with four arms, on which the deflecting magnets can be placed. The pillar also carries a long arm, to which is fixed the telescope and scale. On the top of the pillar is a metal damping ring carrying a tube with torsion head, from which is suspended the magnet. To use the instrument, it is set up with the axis of the damping coil in a direction north and south, and, having been levelled, the frame carrying the four deflection magnets is brought into such a position that the north pole of the magnet points to the east. This position is then marked. The frame is then turned round until the north pole points west; this is effected by making the magnet with two polished surfaces on opposite sides; on turning the frame with the deflecting magnets, the second mirror is seen through the telescope in the same position as was before occupied by the first mirror. This second position of the frame is also marked. The frame can now be placed in any intermediate position, and so long as the earth's force remains the same the scale reading will always show the same deflection. To carry out a comparison the following steps are necessary:—The magnetometer is set up as described at the one place of observation, and the frame is brought to the first mark; the position of the needle is now read on the scale; the horizontal circle or frame is now turned round to the second mark, and the position again read. This will show a difference of n^1 divisions with the first. The same operations are repeated at the second place, and n divisions are found for the difference.

If $2\,\phi$ is the angle through which the horizontal circle is rotated, A the distance from mirror to scale in one instance, and A_1 the distance in the other, we shall have,

$$\frac{H^1}{H} = 1 + \frac{\tan.\,\phi}{4}\left(\frac{n^1}{A^1} - \frac{n}{A}\right)$$

or

$$\frac{H^1 - H}{H} = \frac{\tan.\,\phi}{4}\left(\frac{n^1}{A^1} - \frac{n}{A}\right)$$

if $A = A^1$, or the distance from the mirror to the scale is made the same in both series of observations

$$\frac{H^1 - H}{H} = \frac{\tan.\,\phi}{4\,A}\,(n^1 - n)$$

It is necessary to correct the values found from the above equation for changes of temperature, which affect the magnetism of the needle. To do so we have to add a term, so that the complete expression will be

$$\frac{H^1 - H}{H} = \frac{\tan.\,\phi}{4\,A}\,(n^1 - n) + \mu\,(t - t^1)$$

The coefficient μ may be determined experimentally by making two observations at the same place, with the room at two different temperatures.

The following table shows the ratio $\frac{H^1}{H}$ for two positions in the

Observatory of the Physical Institute, at Wurzburg, one being in the middle of the building and the other near the brick wall. In these experiments $\phi = 35·3°$, A = 2m., so that $\frac{\tan.\phi}{4A} = 0·000069$.

			Mean.	
Oct. 7.	n	= 46·7 – 51·0	48·8	$\frac{H^1}{H}$ = 1·0003
	n^1	= 52·2	52·2	
Oct. 8.	n	= 32·1 – 33·6 – 33·3 ...	33·0	$\frac{H^1}{H}$ = 1·00028
	n^1	= 35·3 – 37·0	36·1	
Oct. 9.	n	= 29·3 – 30·9	30·1	$\frac{H^1}{H}$ = 1·00017
	n^1	= 32·0	32·0	

K. VIERORDT—MEASUREMENT OF THE DECREASE OF SOUND IN THE TELEPHONE.

(Annalen der Physik und Chemie, B. XIX. H 1, No. 5, 1883, pp. 207-213.)

Two of Siemens and Halske's telephones, of the latest pattern, were placed in two rooms so far apart that no direct sound was transmitted, and were connected up in simple circuit. The first telephone was placed 3 cm. above a tin plate 222 mm. by 172 mm. and 8·2 mm. thick, lying on a table. The second telephone was fixed in a stand.

While the experimenter listened at the second telephone, taking care that his ear did not touch it, an assistant let fall lead bullets from varying heights on to the tin plate below the first telephone. The source of sound was thus the shock of the bullet on the tin plate. Starting with a heavy bullet and a considerable fall, the assistant gradually decreased both, as directed through the telephones by the experimenter at the other end, until a sound was produced under the first telephone which was only just audible in the second. The experimenter now placed his ear at the same distance, 3 cm. above the tin plate, as was before occupied by the first telephone, and the assistant again let fall bullets from various heights until a point was reached at which the sound, heard thus directly, was only just audible. If we call the former sound S^1, and the second S, the loss of sound by transmission will be $S^1 - S$.

In an experiment tried at night, when all was quite still, a sound was just heard in the second telephone, when a bullet weighing 1542 mg. was let fall from a height of 7 mm. on to the tin plate. The strength of the sound was then calculated to be $S_1 = 1549 \times 7^{0·62} = 5176$ mg. mm.

When listening directly a sound was just heard, when a bullet weighing 2·4 mg. was let fall from a height of 12 mm. Hence $S = 2·4 \times 12^{0·62} = 8·95$.

Hence when expressed in units of sound of the tin plate, which weighed 2406 g., the loss of sound was 5,176 – 8·95 = 5,167·1. It should be added that the exponent of the height of the fall was found by Oberbeck's method. Two lead bullets respectively 2,073 mg. and 888 mg., hence $\frac{P}{p} = 2·334$, gave the coefficient 0·6196, or in round numbers 0·62 in the one case; and two other bullets respectively 17·5 mg. and 8·75 mg., $\frac{P}{p} = 2$, gave 0·529, or nearly 0·53.

E. EDLUND—RESEARCHES ON THE CHANGES OF TEMPERATURE AT THE ELECTRODES OF A VOLTAMETER WHEN A CURRENT PASSES.

(*Annalen der Physik und Chemie, B. XIX., H. 2, No. 6, 1883, pp. 287-299.*)

In the year 1869 the author propounded the following law:—If a current traverses a battery in the same direction as that produced by the battery itself, a quantity of heat disappears which is proportional to the product of the E.M.F. by the current; if the current flows in the opposite direction, a quantity of heat is developed also proportional to the same product. When in a closed circuit a current is produced by one or several E.M.F., these forces consume a quantity of heat which is equal to the total heat produced by the current circulating through the resistance of the whole circuit. The whole quantity of heat generated by the current is therefore nil, since the quantity produced by the passage of the current is equal to that absorbed by the E.M.F.

The subject having been investigated by Braun, Thomsen, Hoorweg, and others, the author undertook a series of experiments by which he was able to determine whether the quantity of heat used up by the E.M.F. was greater or less than that set free by the chemical actions.

The electrolyte was contained in an oblong wooden vessel 100 mm. long, 40 mm. wide, and 40 mm. deep. The two electrodes just fitted inside the box, and were placed close up to the ends. The box was carefully levelled, so that the liquid might have everywhere the same depth. If a current, i, was passed through the box, a quantity of heat, $g i^2$, was produced in the liquid owing to its resistance, g being a constant. At the anode a salt would be formed: for instance, with a solution of sulphate of copper and a copper plate, sulphate of copper, and a quantity of heat represented by $k i$, where k is another constant, would be produced. But the anode is also the seat of an E.M.F., which, since it acts in the same direction as the current, requires a quantity of heat, $e i$, where e is the E.M.F. At the kathode, where, say, copper is deposited, a quantity of heat $- k i$ is absorbed, but here also an E.M.F. is set up, which acts in the opposite direction to the current, and will also be represented by $e i$. The sum of all the heat actions at the anode will therefore be $g i^2 + k i - e i$, and at the kathode $g i^2 - k i + e i$.

The heat was measured by means of a thermopile constructed of iron and German silver, coated with paraffin and immersed in the bath, so that the junctions were in close proximity to the electrodes. The current was derived from a battery of 5 Bunsen cells, and the thermopile was joined up to a mirror galvanometer.

If the current entered at the electrode A, there was a production of heat equal to $g i^2 + k i - e i$, and the rise of temperature measured by the thermopile would be $f (g i^2 + k i - e i)$, where f is a constant. At the other electrode, B, a rise of temperature, $f^1 (g i^2 - k i + e i)$, would occur. If these two quantities are not equal, the galvanometer would show a deflection, a, which would be proportional to the the difference between them. Hence

$$a = f (g i^2 + k i - e i) - f^1 (g i^2 - k i + e i).$$

On reversing the current, a deflection, b, would be obtained, such that

$$b = f^1(gi^2 + ki - ei) - f(gi^2 - ki + ei).$$

Hence

$$ki - ei = \frac{a + b}{2(f + f^1)}.$$

When the sum $a + b$ is positive, the heat produced by the chemical actions is greater than that consumed by the E.M.F.; and when $a + b$ is negative, the reverse is the case.

The results obtained may be conveniently tabulated as under:—

Experiment No.	Electrodes.	Solution.	Current ampères.	$\frac{a+b}{2(f+f^1)}$
1	Copper	Concentrated sulphate of copper	0·015	+ 35·0
2	"	" "	0·033	+ 74·7
3	Amal. zinc.	Concentrated sulphate of zinc	0·050	+ 23·45
4	Silver	Concentrated sulphate of silver	0·004	– 2·96
5	"	" "	0·014	– 9·12
6	"	Fresh sulphate of silver	0·006	– 5·57
7*a*	"	Dilute nitrate of silver	0·044	– 18·75
7*b*	"	" "	0·023	– 9·0
8	"	Concentrated nitrate of silver	0·039	– 6·89
9	Copper	Concentrated acetate of copper	0·006	+ 11·9
10	Cadmium	Concentrated acetate of cadmium	0·015	+ 20·5
11	Lead	Concentrated acetate of lead	0·013	+ 3·1
12	Silver	Concentrated acetate of silver	0·023	– 6·66

A glance at the above table shows at once that the E.M.F. between silver and silver salts requires more heat than is set free in the chemical actions involved, while with the other metals the reverse is the case.

The first three experiments are of interest in relation to the action of the Daniell cell. The E.M.F. of this cell, which corresponds to the difference of the E.M.F. between zinc in zinc sulphate and of the E.M.F. of copper in copper sulphate, requires just the same quantity of heat as is set free by the chemical actions. Since, according to experiments 1 and 2, the E.M.F. between copper and copper sulphate needs a less qauntity of heat than is produced by the formation of the copper sulphate, therefore the E.M.F. between zinc and zinc sulphate must require a less quantity of heat than is produced by the formation of this latter salt; which is verified by experiment No. 3.

TERQUEM—A CALLAUD CELL WITH VERY LOW RESISTANCE

(*Beiblätter, T. VII., No. 4, p. 306.*)

A glass vessel, 0·16 m. wide and 0·23 m. high, containing 5 litres, is provided at the bottom with a tube closed by a cock, into which a glass tube, bent upwards, is placed. At the top of the cell is a brass cross resting on the edge, in which is placed a spiral coil of thick sheet zinc weighing 3 kg. A funnel passes through this spiral. On the bottom of the cell lies a copper plate, to the centre of which is attached a copper wire passing up through an inverted lamp chimney of glass. The cell is filled with a 60 per cent. solution of pure crystallised sulphate of zinc; in the lamp chimney are placed crystals of sulphate of copper, after a small quantity of the above solution of sulphate of zinc mixed with a saturated solution of sulphate of copper has been poured in. Small strips of zinc hanging down from the brass cross into the liquid prevent the deposition of copper on the zinc spiral. The resistance of the cell with a 60 per cent. solution of zinc is 1·58 ohm.

A certain quantity of liquid is drawn off daily into two flasks filled with granulated zinc, which precipitates the copper, and the liquid can then be poured back into the cell through the upper funnel.

SALCHER—AN ALTERATION IN WEBER'S MAGNETOMETER.

(*Beiblätter, T. VII., No. 4, p. 310.*)

A horizontal rod is made movable on a vertical pivot, which is supported on a heavy foot, and carries a horizontal pulley with a driving band, which is made concentric with the pivot by a screw. The graduated circle with the needle is fixed to the pulley also concentrically. A square block can be slid along the rod and clamped in any position by a set screw. This block carries the vertical axle of a second pulley similar to the former, and which is provided with two brass clamps for holding the deflecting magnet. The driving band, which passes round both pulleys, is best made of india-rubber, so as to allow of stretching.

Owing to this arrangement, if the rod is turned round its axis, the deflecting magnet is turned through an equal angle, and consequently always remains parallel to its original position.

G. DE LUCCHI—THE EFFECT OF MAGNETISATION ON THE CONDUCTIVITY OF IRON IN THE AXIAL AND TRANSVERSE DIRECTIONS.

(*Beiblätter, T. VII., No. 4, p. 314.*)

A number (6 to 23) of circular iron discs, 249 mm. in diameter and 0·315 mm. thick, were superposed, but insulated from each other. The corresponding extremities of the diameters of the discs lying over each other were each joined by an iron wire 40 cm. long and 0·8 mm. diameter, soldered to them, so that at the top and bottom two wires remained free. The same arrangement was made with the second set of diameters at right angles to the former. The whole apparatus was thoroughly well insulated, and covered with a wooden box, the side of which was equal to the diameter of the discs. From the middle points of the sides of the wooden frame two strips of wood projected at right angles, to which the iron wires were fixed. A copper wire insulated with india-rubber was coiled round the whole framework, and its ends were brought into mercury cups. The winding was so arranged that the discs were magnetised axially in the direction of one diameter, and transversely in the direction of the second diameter, which was at right angles to the former. The resistances of the discs and of the iron wires in the one or the other direction were measured on a Wheatstone bridge, with which differences of $\frac{1}{7250}$ could be determined. The circuit was only closed for an instant, and intervals of time were allowed between the observations, so as to avoid all heating.

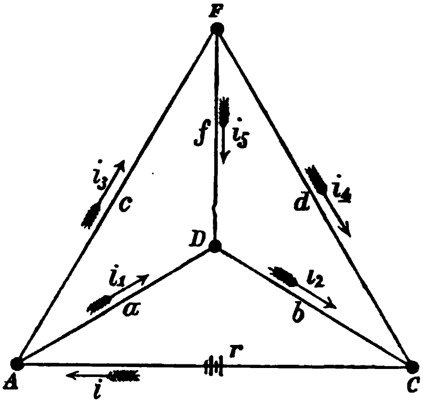
According to these experiments, the resistance is increased by magnetisation in an axial direction, and diminished by magnetisation in an equatorial or transverse direction, as had been already shown by Beetz: the change due to the magnetisation is about $\frac{1}{2765}$.

H. DISCHER—MNEMONIC RULE FOR THE WHEATSTONE'S BRIDGE.

(*Elektrotechnische Zeitschrift, T. IV., H.* 5, *Mai*, 1883, *pp*. 198, 199.)

The author proposes a mnemonic rule, by means of which the current in any of the six arms of a Wheatstone bridge can be written down at once.

The following figure shows the diagram proposed as basis of the plan:—



The equations will all take the form $i = \mathrm{E} \times \frac{m}{n}$, where m will vary with each equation, but the denominator, n, will be common to all. It is first necessary to find n. To do this, form all possible combinations of the six resistances, r, a, b, c, d, f, taken three together. We shall have $\frac{6 \cdot 5 \cdot 4}{1 \cdot 2 \cdot 3} = 2$ combinations, viz.,

$r\,a\,b$
$\underline{r\,a\,c}$
$r\,a\,d$
$r\,a\,f$
$r\,b\,c$
$\underline{r\,b\,d}$
$r\,b\,f$
$r\,c\,d$
$r\,c\,f$
$r\,d\,f$

$a\,b\,c$
$a\,b\,d$
$\underline{a\,b\,f}$
$a\,c\,d$
$a\,c\,f$
$a\,d\,f$

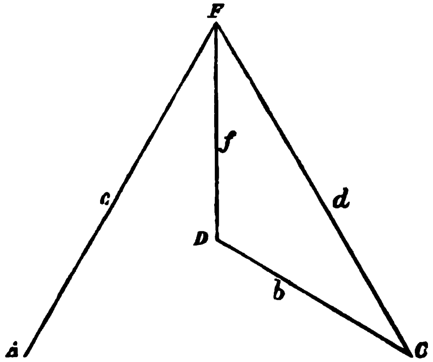
$b\,c\,d$
$b\,c\,f$
$b\,d\,f$

$\underline{c\,d\,f}$

Now strike out those terms formed by three resistances which have one of the four points A, C, D, F of the figure as common meeting point (underlined in above list). The sum of the sixteen remaining products is the common denominator, n.

In order to find the numerator, m, for any branch, draw the diagram, omitting the branch containing the source of electricity and the branch the current in which is required. With the remaining resistances form all possible combinations taken *two* together, and strike out those terms formed by the *two* resistances which have one of the points of the reduced diagram as common meeting point. If *three* resistances meet in a point, the corresponding terms are not to be struck out.

For example, suppose we require to find the value of i_1, the reduced diagram will have the form,



With the four resistances, b, c, d, f, we can make $\frac{4 \cdot 3}{1 \cdot 2} = 6$ combinations, viz.,

$b\,c$, $b\,d$, $b\,f$, $c\,d$, $c\,f$, $d\,f$.

Of these, $b\,d$ have the common point C, $b\,f$ the common point D; omitting them, therefore, we shall have $i_1 = \text{E} \times \frac{bc + cd + cf + df}{n}$ as the value of the current in the branch required.

Remark that in i_5, which is the current in the galvanometer branch, the *difference* of the products is to be taken, not the sum.

Proceeding as in the above example, we shall obtain the six equations,

$$i = \text{E} \times \frac{ab + ad + af + bc + bf + cd + cf + df}{n}$$

$$i_1 = \text{E} \times \frac{bc + cd + cf + df}{n};$$

$$i_2 = \text{E} \times \frac{ad + cd + cf + df}{n};$$

$$i_3 = \text{E} \times \frac{ab + ad + af + bf}{n};$$

$$i_4 = \text{E} \times \frac{ab + af + bc + bf}{n}$$

$$i_5 = \text{E} \times \frac{ad - bc}{n};$$

n all of which

$$n = rab + rad + raf + rbc + rbf + rcd + rcf + rdf + abc + ab \\ + acd + acf + adf + bcd + bcf + bdf.$$

W. HALLWACHS—THE ELECTRO-MOTIVE FORCE, RESISTANCE, AND EFFICIENCY OF ACCUMULATORS.

(*Elektrotechnische Zeitschrift, B. IV., H. 5, Mai*, 1883, *pp.* 200-208.)

The experiments were carried out with the secondary batteries of Tommasi, Bréguet, and Otto Schulze.

The Tommasi cell is made up of very thin lead plates, which are kept from touching by pieces of ebonite. The cell is "formed" as in Planté's method. Bréguet's cell is just like an original Planté cell, being made up of two plates of lead kept apart by strips of india-rubber, and then rolled up spirally. Two forms of Schulze's cell were used. In the first, lead plates are placed in a box, and the alternate ones are connected together. The formation is aided by the spreading of minium over the plates, which are made like a gaufre, so as to retain the minium better. The second form of cell was built up of 80 plates, which, after being scratched, were covered with flowers of sulphur and exposed to heat, so as to become coated with a layer of lead sulphide.

The liquid in all cases was made up of ten parts water to one part of sulphuric acid.

The quantities measured both during charge and discharge were the resistance of the cell, the strength of the current, and the difference of potential at the terminals of the closed cell. The current was measured by means of a galvanometer by Sauerwald, which was joined up in a shunt circuit. The readings of the galvanometer were reduced to ampères in the following way —The current from a Daniell cell, the E.M.F. of which had previously been determined by an electrometer, was sent through the main circuit, the resistance of which being known the current could readily be calculated. The difference of potential was measured by means of one of Mascart's quadrant electrometers. The resistances were measured on a Wheatstone bridge, using alternating currents and a telephone in place of a galvanometer.

The E.M.F. was calculated from the formulæ:

$$E = E^1 - I\,R \text{ (charge)},$$
$$e = e^1 + i\,r = i\,(l + r) \text{ (discharge)},$$

where r and R are the resistance of the cell, and l that of the outer circuit. From the observations on the E.M.F., it was found that this was dependent on the current. If we call a current positive which charges the cell, and negative when it discharges, so that the open cell corresponds to no current, then in passing from positive values through zero to negative values, the E.M.F. decreases. The stronger the discharge current, the more rapidly does the E.M.F. fall off. With a cell as completely discharged as possible, the E.M.F. is about 0·4 to 0·5 volts, but this almost at once increases on charging to 2 to [illegible] volts. If the charging current decreases towards the end, the E.M.F. may decrease also. On commencing the discharge, the E.M.F. will be about 2 volts, and this will at first fall very slowly, and then more rapidly. The amount of E.M.F. varies also with the use which has been made of the cell.

The internal resistance of the cell on charging decreases very suddenly to a certain value, at which it remains constant for the remaining time of charge. On discharging the cell, the resistance at first remains at this constant value, and then regularly increases.

When a cell is being charged, an amount of work is being expended by the source of electricity, which is represented by

$$L = \int I E^1 \, d T = \int I E \, d T + \int I^2 R \, d T = L_e + L_w,$$

where L_e is the electrolytic work, and L_w the heat produced in the cell according to Joule's law. On discharging through an external resistance, s, we shall have

$$l = \int e i \, d t = \int i^2 s \, d t + \int i^2 r \, d t = l_s + l_w,$$

where l_z is the work done in the outer circuit, and l_w the heating effect.

Then the efficiency $$N = \frac{l}{L_e}$$

The efficiency, N, increases usually with the use of the cell. The maximum efficiency is apparently obtained when the charging current is between 4 and 11 ampères. So far as the author has experimented, he finds that the duration of the charge has very little effect on the efficiency. The efficiency seems to be quite independent of the external resistance during discharge. The highest efficiency obtained by the author in his experiments was N = 0·5. This was obtained with the one of Schulze's cells in which the plates had been coated with minium: the cell had been charged with a current of 8·3 ampères during 4 hours, and was then discharged through 0·5 ohm.

The ratio, $\frac{L_w}{L}$, *i.e.*, the heat developed on charging, to the total energy spent in charging is greater for a rapid charge than for a lengthened one. Again, $\frac{L_w}{L}$ is especially large for very strong and for very weak currents. It is least for those values of I for which N is greatest. The ratio, $\frac{l_w}{l_s}$, *i.e.*, the heat developed on discharging, to the work done in the outer circuit is nearly inversly proportional to s, the external resistance.

The greatest amount of work was given by Schulze's minium cell, *viz.*, 175,000 volt-ampère-seconds = 18,000 kgm. The weight of the cell was 8·1 kg., of which 6 kg. were lead; hence the cell gave about 3,000 kgm. per kg. of lead. This cell, during the experiments, gave out in all about 1·2 million kgm. of work, which would correspond to a daily charge and discharge for a month; before this, however, it had been daily used for lighting purposes during two months, so that on the whole it lasted three months of daily use.

The following table exhibits some of the results obtained:—

Schulze Cell with minium.

Charge.			Discharge.				
Time in hours.	Current ampères.	L_e in watts per sec. = 10^3 ×	External resistance. Ohms.	Time in minutes.	l in watts per sec. = 10^3 ×	Current ampères.	H.
10	6·9	800	0·22	123	224	10·1	0·28
1	19·4	110	0·132	5	7·1	13·2	0·06
4	3·9	122	0·138	15	26	13·0	0·21
4	17·1	533	1·136	590	130	1·8	0·24
4	1·1	33·7	1·137	12	2·0	1·7	0·06
13	1·0	95·3	1·137	48	8·6	1·6	0·09
4	6·9	225	1·137	178	33·7	1·6	0·105
4	7·2	235	1·137	435	87·8	1·7	0·37
4	10·8	363	1·136	647	126	1·7	0·35
7	7·4	434	1·136	900	175	1·7	0·40
1	6·7	51·3	1·142	128	23	1·6	0·45
4	8·3	276	0·5	303	137	3·8	0·50
4	4	133	0·135	36	62	13·5	0·47
4	6·6	219	0·195	93	108	9·4	0·49
4	4	132	0·135	39	63·7	13·2	0·48
4	4·2	141	0·118	27	39·5	13·2	0·28
Bréguet Cell.							
3	6·1	161	0·185	7	13·6	14·7	0·08
Tommasi Cell.							
4	4·1	138	0·135	21	31·6	12·6	0·23
4	4·0	126	0·113	23	40·8	14·3	0·32
Schulze Cell with sulphide.							
2·5	21·4	434	0·297	131	87·0	6·0	0·20
4	3·6	109	0·118	25	44·1	14·8	0·405

JAMIN and MANEUVRIER—THE ELECTRIC LIGHT IN AN ATMOSPHERE OF CARBON BISULPHIDE.

(*Elektrotechnische Zeitschrift, B. IV., H. 5, Mai,* 1883, *p.* 221.)

The authors used two carbons at right angles to each other, which could be readily moved from or to each other. If a vacuum is produced in the vessel, so that there is no longer any arc, but only a slight glow, and then a few drops of carbon bisulphide are introduced, sufficient to increase the pressure by about 0·05 m., the arc can be again established, and is then of an unbearable brilliancy. Observed through a dark glass, the arc appears in the form of a horse-shoe about 0·05 m. high, surrounded by a long flame, which breaks into points above. The carbon points are red hot, but the light from the arc is bluish-green, such as the flame given by copper. The intensity of the light increases with the pressure in the vessel; but as the resistance increases at the same time, the light goes out, but can be reproduced by making the carbons touch. The spectrum of the light shows four bands, in the red, yellow, green, and violet, of which the green is the brightest: all are very distinct. A dark brown deposit is formed on the glass, which is probably a compound of carbon and sulphur. The authors suggest that the light might be useful as a signal.

F. EXNER—THE ELECTRO-MOTIVE FORCE OF FRICTIONAL MACHINES.

(*Centralblatt, B. V., No.* 9, 1888, *pp.* 207, 208.)

The measurements were undertaken chiefly to determine whether the E.M.F. is a function of the speed of rotation. The measurements were made with an absolute electrometer on Thomson's plan. The lower fixed plate could be raised or lowered by means of a micrometer screw, and was carefully insulated; the upper plate was suspended from one arm of a balance, and was provided with a guard ring, both being put to earth. Then, on joining up the lower plate to an electrical machine, the upper one will be attracted, and we shall have

$$V = d\sqrt{\frac{8\pi m g}{S}},$$

where V is the difference of potential in absolute electrostatic units, d is the distance between the two plates which is measured by the micrometer screw, S the area of the movable plate, m the balancing weight, and g the acceleration due to gravity. The following results were obtained:—

Holz Machine.

Revolutions per second.	*d.* mm.	*m.*	*m.*	*m.*	E.M.F. Daniells.
		With Leyden jar. Grammes.	Without Leyden jar. Grammes.	Mean Grammes.	
1	10·5	1·52	1·55	1·535	9,540
3½	10·5	7·10	7·15	7·125	20,580
Winter Machine.					
1	11·37			1·92	11,610
3	11·37			1·92	11,610

The use of the Leyden jar seems to have no appreciable effect; but in the case of the Holz machine the velocity has a marked effect, though no change was produced in the Winter machine. It would seem, therefore, as though the E.M.F. produced by friction is a constant for each pair of bodies, as is the case when two bodies act chemically on each other.

PROF. RITTERSHAUS—THE KINEMATICS OF DYNAMO MACHINES.

(*Centralblatt, B. V., No.* 11, 1883, *pp.* 253-257.)

The author compares dynamo machines with steam-engines, the commutator corresponding to the slide valve, or four-way cock, or other arrangement for regulating the steam supply. The currents induced in the separate coils of the machine are always alternating in direction, this being reversed at the moment of the passage of the particular coil through the neutral part of the field; these alternating currents are then collected by the commutator. The commutator in its simplest form—for one coil and two fields—is exactly the equivalent of the four-way cock: two conducting pieces on opposite sides correspond to the steam passages.

If the machine has two coils with two fields, and if these are opposite to each other, so that they pass the neutral diameter simultaneously, the commutator may serve for both. The coils will then be joined up parallel, and the brushes form the connection with the outer circuit. This corresponds to a two-cylinder engine, the pistons of which are united by a cross head, and are regulated by a single four-way cock. In compound steam-engines there must be one six-way cock or two four-way cocks, and these engines may be looked upon as connected in series, the steam passing from the high-pressure cylinder to the low-pressure cylinder. In the same way the coils of a dynamo machine may be coupled in series by means of two commutators.

If there are more than two coils, a separate commutator may be provided for each coil or for each pair of coils, as has been done by Brush; or the several currents may all be united in one common distributing apparatus, as has been done for steam in Brotherhood's three cylinder engines. The author traces similar analogies in other machines.

DR. A. LEMAN—APPARATUS FOR REGISTERING AUTOMATICALLY THE STRENGTH OF EARTH CURRENTS.

(*Zeitschrift für Instrumentenkunde, B. III., H.* 4, *April*, 1883, *pp.* 132-135).

The apparatus was designed so that by simply shunting it into the circuit of a telegraph wire a diagram of the varying intensity of the earth currents should be traced out. Owing to these currents being usually very weak, it was not possible to employ any mechanical arrangement, and recourse was had to photography. The plates used—bromide of silver gelatine plates—being excessively sensitive, all extraneous light is carefully shut out by enclosing the whole apparatus in a wooden case. The general arrangement may be seen from Fig. 1.

A bundle of rays from the lamp passes through a small aperture in a diaphragm closing the end of the tube, and, after traversing the latter, strikes a prism from which it is reflected on the mirror of a Siemens' dead-beat galvanometer. The reflected ray from the mirror is focussed by the lens on to the vertical screen supporting the gelatino-bromide plate. The plate is placed in a groove in a wooden box before beginning the observations; a screen can be slid up and down in the groove in front of the plate, as in an ordinary camera. The experimenter can introduce his hand through an opening covered by a black cloth, and can raise the slide to the top of its frame; it is then allowed to descend regularly and slowly, being controlled by a clock. In front of the slide is a horizontal slit, which allows the ray of light to pass. The descent of the slide is at the rate of about 80 mm. per hour. As the sensitive plate slowly moves downwards in front of the slit, the spot of light affects it, and the photograph will show any deflections which the mirror has made to right or left of the medial line. Time signals are given by means of the diaphragm at the front end of the tube; in this diaphragm are pierced three equidistant horizontal holes. Generally the light is allowed to pass only through the centre hole; but at fixed intervals of time (say every half-hour)

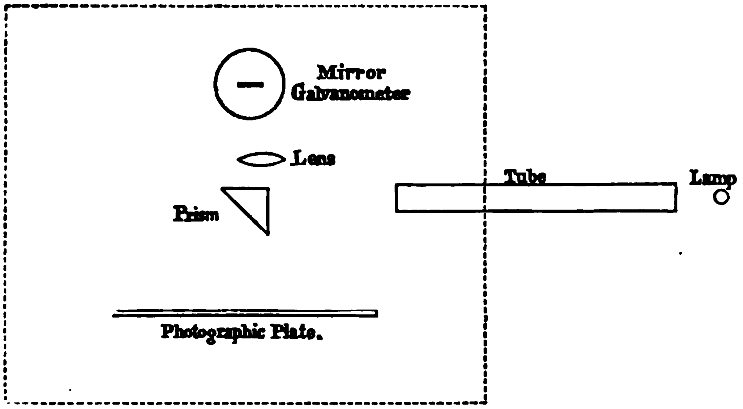


FIG. 1.

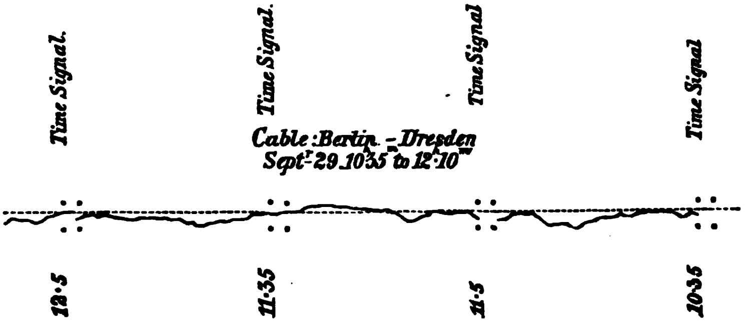


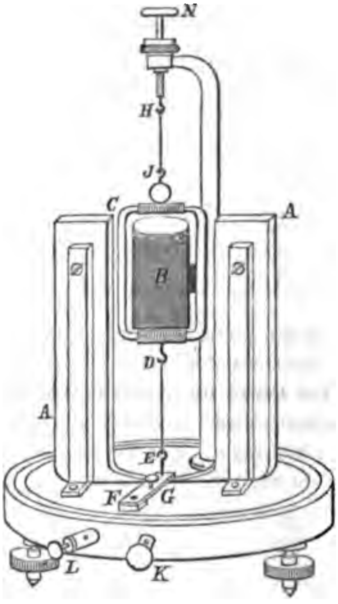
FIG. 2.

the two side holes are opened, and their images then appear on the photograph on each side of the medial line. It is thus easy to measure the duration of any earth current. Fig. 2 is a fac-simile of a curve traced by the apparatus, showing the earth currents on the underground cable between Berlin and Dresden on the morning of 29th September last.

DEPREZ and D'ARSONVAL—DEAD-BEAT GALVANOMETER.

(*Dingler's Journal*, *B.* 248, *H.* 1, *März* 4, 1883, *p.* 24.)

A horse-shoe magnet, A, is fixed with its legs vertically upwards in the base plate. Between the legs is a rectangular frame, C, which is formed of a considerable number of convolutions of very fine wire.



Two wires, J H and D E, of silver or copper, are fastened to the frame; of these, J H is attached to the torsion head, N, which can be either rotated or raised; the other wire, D E, is attached to a flat spring, F, at the base of the instrument, the tension of which can be regulated by the screw, G. These wires serve to conduct the current to the coil, as well as for supports. The deflection is read in the usual way by means of the small mirror, J. B is an iron tube, which intensifies the magnetic field. The instrument is very dead-beat, as any movement of the coil in the very intense magnetic field induces a current, which at once tends to annul the movement

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ELECTRIC LIGHT FOR SUBMARINE OPERATIONS.

(*Dingler's Journal*, *B.* 248, *H.* 1, *März* 4, 1883, *p.* 42.)

In the "Annales des Ponts et Chaussées" (1882, B. 4, p. 245) is a note by the engineer Mr. Lechalas, on the use of electric light in submarine operations. The lights were used in the caissons employed in the construction of the quay walls of Antwerp. Swan lamps were chosen, as the workmen required to shift the lights about according to the exact position of the work going on, and were enclosed in strong glass lanterns, protected from shocks by a bronze wire screen. From 8 to 10 lamps were used in each caisson, the dimensions of which were 20 m. long, 9 m. wide, and from 2·5 m. to 6 m. high. The necessary current for the 30 lamps in 3 caissons was furnished by two Faure accumulators, each consisting of 20 cells, which were charged on shore by a Gramme machine.

Dr. A. SLABY—ELECTRICAL TRANSMISSION OF POWER.

(*Dingler's Journal*, *B.* 248, *H.* 3, *April* 18, 1883, *pp.* 95-103.)

In a paper read before the Verein zür Beförderung des Gewerbfleisses, the author has treated in a very clear and complete way the above question. He has, however, not introduced any new matter, but gives a general résumé of the subject, using the well-known equations for calculating the return of a system of two machines. The lecture was elementary, being intended for engineers not specially acquainted with electrical science, and does not call for an extended abstract.

O. FROLICH—SIEMENS' TORSION GALVANOMETER.

(*Proceedings of the Physical Society of Berlin*, *No.* 5, *April* 19, 1883, *and Elektrotechnische Zeitschrift*, *B. IV.*, *H.* 5, *Mai*, 1883, *pp.* 195-198.)

The instrument, a short notice of which, accompanied by an illustration, appeared in the *Electrical Review* for June 10, 1882, consists of two vertical and parallel coils of wire, between which a so-called "bell-magnet" is suspended from a spiral spring. The deflection produced by the current in the coils is counteracted by turning the torsion head, to which one end of the spiral spring is attached, so as to bring the pointer back to zero. The angle of torsion of the spiral spring is proportional to the strength of the current traversing the instrument, or, since its resistance is a constant quantity, to the difference of potential at the terminals.

For currents of more than 5 ampères the coil has a resistance of 1 ohm, while for weaker currents of less than 5 ampères the resistance is 100 ohms. The instrument is made in two forms, vertical and horizontal, and with each form a box of resistances is supplied, by means of which E.M.F. from 0·001 volt to 1,700 volts can be measured on the same instrument.

The galvanometer is calibrated by joining it up in circuit with two thermopiles of each 50 elements, the one of German silver and copper, and the other of German silver and iron. The current strength is calculated on the assumption that 1 ampère deposits 3·96 grammes of silver per hour. The instrument is so adjusted that the angle of torsion is very nearly equal to the difference of potential in volts; but a table of corrections is made out for each instrument, which permits of the exact value being obtained.

To measure the difference of potential between any two points of a circuit, it is only necessary to join up the galvanometer and the box of resistances between the two points, and the angle of torsion gives at once the value sought. In actual work, it is advisable at first to employ the highest resistance in the box, and then to reduce this until the required degree of sensibility is obtained. The box of resistances is joined up in series with the galvanometer, and not as a shunt.

Besides the above use, the galvanometer also serves to determine E.M.F. current, resistance, and work. Suppose it is required to know the E.M.F. of a dynamo machine, measure the difference of potential at the terminals, and add or subtract the product of the current by the resistance of the machine, accordingly as it is used as a generator or a motor, or expressed in symbols

$$E = P \pm I R.$$

The strength of the current is obtained by dividing the difference of potential by the resistance of the circuit between the points at which the difference of potential is measured.

Conversely, the resistance may be obtained by dividing the difference of potential by the current strength.

If $P =$ difference of potential in volts, and $I =$ current strength in ampères, then the work in horse-power $A = C P I$, where $C = 0{\cdot}00136$.

Dr. H. ARON—ACCUMULATORS.

(*Proceedings of the Physical Society of Berlin, No. 6, April* 19, 1883, *and Elektrotechnische Zeitschrift, B. IV., H.* 2 *and* 3, *February and March*, 1883, *pp.* 58 *and* 100.)

The author has communicated to the *Elektrotechnische Verein* a very exhaustive account of his researches on secondary batteries, the results obtained in which may be shortly resumed.

1. Spongy lead, precipitated from lead acetate by zinc and compressed into plates, becomes at once active if used as the negative plate, but not if it is the positive. It was owing to this that the author's attempts to form secondary batteries in the beginning of 1880, before Faure had brought forward his plan, were abortive.

2. Rolled lead can be coated with a layer of disintegrated lead in a comparatively short time by means of an electric current, when it is placed in diluted sulphuric acid, if about one per cent. of nitric acid is mixed with the sulphuric acid.

3. The positive plates of rolled lead, whether they themselves form the electrodes or only serve as supports to the spongy lead deposited on them, become disintegrated to a depth of about half a millimètre on each side, and accordingly they should have a thickness of at least 2 mm.; the connections should be made by means of lead strips 5 mm. thick, soldered on with tin solder.

4. Very coherent plates may be obtained by covering them with a mixture of minium and collodion: this compound is a good conductor. In order to maintain the paste in place, the plates should be wrapped round, while still moist, with woollen threads.

5. As to the action at the negative plate, from the calorific value of the chemical action it is certain that the reduced lead is converted on discharge into lead sulphate. Suppose x is the calorific value of the chemical action on the negative plate, and y that at the positive, while E is the E.M.F. of the cell in calorific measure, then $x + y = E$. It was found that $E = 1{\cdot}78$ Daniells $= 89{,}230$ thermal units (1 Daniell $= 50{,}130$). In order to find y, the reduced lead plate was replaced by a copper plate, forming, in fact, a Sutton's cell—copper in dilute sulphuric acid as negative plate, and peroxidised lead as

positive. From the blue colour of the liquid, it was evident that the action of the negative plate resulted in the formation of copper sulphate, the thermal value of which is 55,960. The E.M.F. of such a cell was found to be 1·31 Daniell = 65,670 thermal units, hence

$$\left.\begin{aligned} 55{,}960 + y = 65{,}670 \therefore y &= 9{,}710 \\ x &= 79{,}520 \end{aligned}\right\}$$

whilst, according to J. Thomson, the calorific equivalent of lead sulphate is 73,800. The difference may be accounted for, on the one hand, by the uncertainty attending the calculation of the E.M.F. of a combination from the thermal values of the chemical actions, and, on the other hand, the lead is in a disintegrated state.

6. At the positive plate the brown peroxide is not formed, as has been generally supposed, but a bluish-black compound, which was proved by the chemical tests applied to be a hydrated peroxide (plumbic acid $Pb\,O\,(OH)_2$). This hydrated peroxide is more stable in dilute sulphuric acid than in pure water, by which it is soon decomposed into peroxide and water. If a plate coated with peroxide be placed in a charged cell in place of the one coated with the hydrated peroxide, the cell will not be nearly so constant.

7. The active part taken by the sulphuric acid is evidenced by the increase in the specific gravity of the liquid on charging and the decrease on discharging, amounting to a difference of $\frac{1}{10}$ in the two specific gravities.

8. The resuscitation of a secondary battery may be attributed to the action of the sulphuric acid. The plates first use up the acid in their immediate vicinity, and have to wait until a further quantity has become equally diffused through the liquid.

9. On this account it is advisable not to hinder in any way the free circulation of the liquid.

10. The capacity of a cell may be as much as 3,000 kilogrammètres of electrical energy per kilogramme of the gross weight; but even in the most favourable instances the formula

$$Pb + 2\,H_2\,SO_4 + Pb\,O + H_2\,O_2 = Pb\,SO_4 + Pb\,SO_4 + 3\,H_2\,O$$

shows that only 50·07 grammes per kilogramme of gross weight are really active. It is probable that only the outer surfaces of the lead particles can be active, and as soon as these are covered with a layer of lead sulphate the action stops.

11. The cells with only thin coatings, such as those prepared with the aid of nitric acid, hold their charge only a few days: a gradual discharge takes place, and the positive plate becomes covered with a white film which is found to be lead sulphate; but by means of a further formation the brown peroxide can again be produced. When in a dry state the plates seem to hold their charge better.

12. The plates with thick coatings at first keep their charge better, but afterwards they fall off in capacity of storage. The observations showed that the black mass, which forms brittle hard pieces, had disappeared, and in its place was a light brown pappy substance (it is probable that each particle becomes coated with lead sulphate, whereby its adhesion is spoiled); the

current then passes no longer through the lead sulphate coated particles, but goes direct to the supporting plate.

13. A positive plate prepared in Faure's way from lead sulphate shows that oxidisation only takes place at the places which are in immediate contact with the lead plate, so that, as Gladstone and Tribe have shown, the lead sulphate is in fact converted into lead proxide, but only in the immediate neighbourhood of the lead plate, and not throughout the mass.

LIST OF OTHER ARTICLES.

(*Centralblatt für Elektrotechnik*, 1883, *B. V.*)

No. 10.—Extension of the Telephone—Transmission of Power at the Northern Railway of France—Electrical Measuring Instruments—The Theory of the Micro-Telephone.

No. 11.—Electric-Energy-Meter—Transmission of Power at the Northern Railway of France—Automatic Telegraph—Electric Lighting at the Disconto-Bank in Paris—Apparatus of the Swiss Telephone Company at Paris Exhibition.

(*Dingler's Polytechnisches Journal*, 1883, *B.* 248.)

Heft 1.—W. Lake's Boat, Electrically controlled from the Shore.

Heft 2.—Lewis's Insulator—Deprez's Electric Hammer—Brockie's Arc Lamp.

Heft 3.—Electric Locomotives of Siemens, Dupuy, Felix, and Electric Power Storage Company—Electric Lighting in Textile and Paper Mills.

(*Repertorium der Physik*, 1883, *B.* 19.)

Heft 3.—F. **EXNER**—Some Experiments illustrative of the Contact Theory.

(*Journal für Gasbeleuchtung*, *B.* 26.)

Heft 7.—**Dr. KRÜSS**—The Unit of Light.

(*Deutsche Bauzeitung*, 1883, *B.* 17.)

No. 32.—**Dr. WR. SIEMENS**.—Plan for an Electrical Railway in Vienna.

No. 34.—C. **CÖRPER**—Electric Light and Transmission of Power.

(*Journal Télégraphique, Berne*, 1883, *B.* 17.)

No. 4 —T. **ROTHEN**—The Present State of the Question of Electrical Units—Transmission of Power by the System of Deprez—The Commission on Electrical Units—Historical Notes on Telephones.

(*Schweizerische Bauzeitung* (*Revue Polytechnique*), *Zurich*, 1883, *B.* 1.)

Nos. 15 and 16.—**L. ZEHNDER**—The Origin of Atmospheric Electricity and its Connection with Terrestrial Electrical Phenomena.

(*La Lumière Electrique*, 1883, *Vol.* 8.)

No. 11.—**C. HERZ**—Transmission of Power. **DU MONCEL**—Observations on Microphones. **GUÉROUT**—History of the Telegraph. **COSSMAN**—Applications of Electricity to Railways—The Latest Improvements in the "Sun" Lamp.

No. 12.—**DU MONCEL**—The Two Currents of the Induction Spark. **GUÉROUT**—History of the Telegraph.

No. 13.—**DU MONCEL**—The Two Currents of the Induction Spark. **REGRAY**—Electric Brakes. **GUÉROUT**—History of the Telegraph. **COSSMAN**—Applications of Electricity to Railways.

No. 14.—**DU MONCEL**—The Two Currents of the Induction Spark. **DE MAGNEVILLE**—Electric Lamp of Tihon and Rézard. **COSSMAN**—Applications of Electricity to Railways. **DEJONGH**—The Microphone and the Telephone. **SARTIAUX**—Ducousso's Automatic Train Indicator.

No. 15.—**HERZ**—Transmission of Power. **MERCADIER**—Elementary Theory of Electricity. **SOULAGES**—Lighting of the Eden Theatre at Paris. **GÉRALDY**—Electric Lighting of Theatres.

No. 16.—**DU MONCEL**—Speed Regulators. **MERCADIER**—Elementary Theory of Electricity. **COSSMAN**—Applications of Electricity to Railways. **GÉRALDY**—Parasitic Action of Dynamo Machines. **GUÉROUT**—Siemens' Machine with Outside Armature. **KERN**—Cost of Electric Lighting.

JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

VOL. XII. 1884. No. 50.

The One Hundred and Twenty-fifth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 8th, 1883—Mr. WILLOUGHBY SMITH, President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The names of new candidates were announced and suspended.

The following transfers were announced as having been made on the approval of the Council:—

From the class of Associates to that of Members—

F. Bolton.
Walter Hall.
William McWhirter.
William Oliver Smith.

From the class of Students to that of Associates—

A. P. Chattock.
Francis Elliott Stuart.

It was announced that donations to the Library had been received from the following persons and Institutions, and thanks were awarded by the meeting:—Institution of Civil Engineers, Iron and Steel Institute, Lords of the Committee of Council on Education, Managing Committee of the Vienna Electrical Exhibition, H. M. Patent Office, Radcliffe Library, Royal Engineers'

Institute, Royal Observatory, Royal Society of South Australia, Smithsonian Institution, Society of Engineers, Telegraph Construction and Maintenance Company, University College, and United States Signal Service. Dr. Von Beetz, W. H. M. Christie, Esq. (Astronomer-Royal); P. Cowell, Esq.; Director-General of Russian Telegraphs, Dr. G. Gore, Herr A. Hartleben, Colonel W. Haywood, Mons. N. Hoffmeyer, G. R. Humphrey, Esq., Mons. E. Jacquez, Mons. L. de Locht-Labye, Mons. Melsens, E. Perrody, Esq., H. S. Snell, Esq., and H. Tomlinson, Esq. Mons. Adolphe Cochery, Honorary Member. Le Commandeur E. D'Amico (Local Honorary Secretary for Italy), E. Hospitalier, T. Rothen, and H. W. Spang, Foreign Members. E. B. Bright, Latimer Clark (Past-President), J. Angelo Fahie, J. J. Fahie, Walter T. Glover, Professor Fleeming Jenkin, C. E. Spagnoletti (Vice-President), J. W. Swan, Professor S. P. Thompson, and C. Todd (Local Honorary Secretary for South Australia), Members. John Munro and C. J. Wharton, Associates.

The President: Gentlemen,—It affords me much pleasure to meet you here again, after so long a recess; but that pleasure is somewhat marred by the fact that, since we last met, we have lost one of our most distinguished members—I refer to the untimely death of Mr. Cromwell Fleetwood Varley. It is not for me now to extol the merits of Mr. Varley—that, I hope, will be done by a more competent person than myself; but as a Society we have to accord our sympathy with those friends who are left to mourn their loss, and also to record our regret that Mr. Varley was not spared to work longer with us, or to enjoy the fruits of the harvest the seeds of which he so early sowed and fostered.

There is one other subject which I will mention, though perhaps it is somewhat unnecessary, as it will no doubt be dealt with fully by the Council in their Annual Report—I refer to the slight alteration that has taken place in the formation of the Society since we last met. The success of this Society during the thirteen years it has been established has been so great, that the Council were afraid lest they were erecting a magnificent structure on an insecure foundation; they therefore turned their attention to the subject, and, although it may appear a very simple matter, I can assure you it was a work of some difficulty and labour, and I was agreeably surprised to find those gentlemen so ready to give their valuable time, energy, and even money when required, for the benefit of your Society. The result is that we meet this evening for the first time on a new basis, which I am sure is a good one; and, if you will only continue to elect as members of your Council such men as it has been my pleasure to work with for so many years, I am sure that the Society will continue to flourish as it has hitherto done. I am pleased that Colonel Webber has reminded me that Mr. Bristow, our worthy honorary solicitor, is present this evening, for it is owing to his able assistance that the work of incorporation was got over so well, and I had Mr. Bristow's labours plainly before me when speaking of the gentlemen who had so liberally given their time and energy for the welfare of this Society.

Now, gentlemen, with your permission, I will ask Professor W. G. Adams to kindly preside during the time that I fulfil my promise of reading my paper set down for this evening.

Professor W. G. Adams then took the chair.

The following paper was then read:—

VOLTA-ELECTRIC INDUCTION.

By Willoughby Smith, President.

In my presidential address which I had the pleasure of reading before this Society at our first meeting this year, I called attention, somewhat hurriedly, to the results of a few of my experiments on induction, and at the same time expressed a hope that at a future date I might be able to bring them more prominently before you. That date has now arrived, and my endeavour this evening will be to demonstrate to you by actual experiment some of what I consider the most important results obtained. My desire is that all present should see these results, and with that view I will try, when practicable, to use a mirror reflecting galvanometer instead of a telephone. All who have been accustomed to the use of reflecting galvanometers will readily understand the difficulty, on account of its delicacy, of doing so where no special arrangements are provided for its use; but perhaps with a little indulgence on your part and patience on mine the experiments may be brought to a successful issue.

Reliable records extending over hundreds of years show clearly with what energy and perseverance scientific men in every civilised part of the world have endeavoured to wrest from nature the secret of what is termed her "Phenomena of Magnetism;" and, as is invariably the case under similar circumstances, the results of the experiments and reasoning of some have far surpassed those of others in advancing our knowledge. For instance, the experimental philosophers in many branches of science were groping as it were in darkness until the brilliant light of Newton's genius illumined their path. Although perhaps I should not be justified in comparing Oersted with Newton, yet he also discovered what are termed "new" laws of nature, in a manner at once precise, profound, and amazing, and which opened a new field of research to many of the most distinguished philosophers of that time, who were soon engaged in experimenting in the same direction, and from whose investigations arose a new science, which was called "Electro-dynamics." Oersted demonstrated from inductive reasoning that every conductor of electricity possessed all the known properties of a magnet while a current of electricity was passing through it. If you earnestly contemplate the important adjuncts to applied science which have sprung from that apparently simple fact, you will not fail to see the importance of the discovery; for it was while working in this new field of electro-magnetism that Sturgeon made the first electro-magnet, and Faraday many of his discoveries relating to induction.

Soon after the discovery by Oersted just referred to, Faraday, with the care and ability manifest in all his experiments, showed that when an intermittent current of electricity is passing along a wire it induces a current in any wire forming a complete circuit and placed parallel to it, and that if the two wires were made into two helices and placed parallel to each other the effect was more marked. This Faraday designated "Volta-electric-induction," and it is with this kind of induction I wish to engage your attention this evening; for it is a phenomenon which presents some of the most interesting and important facts in electrical science.

Here are two flat spirals of silk-covered copper wire suspended separately, spider-web fashion, in wooden frames marked respectively A and B. The one marked A is so connected that reversals at any desired speed per minute from a battery of one or more cells can be passed through it. The one marked B is so connected to the galvanometer and a reverser as to show the deflection caused by the induced currents, which are momentary in duration, and in the galvanometer circuit all on the same side of zero; for as the battery current on making contact produces an induced current in the reverse direction to itself, but in the same direction on breaking the contact, of course the one would neutralise the other, and the galvanometer would not be affected. The galvanometer connections are therefore reversed with each reversal of the battery current, and by that means the induced currents are, as you perceive, all in the same direction, and produce a steady deflection. The connections are as shown on the sheet before you marked 1, which I think requires no further explanation.

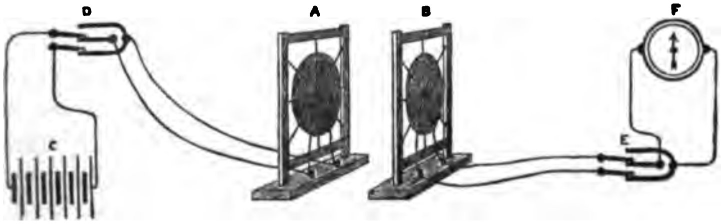


PLATE 1.

Before proceeding, please bear in mind the fact, that the inductive effects vary inversely as the square of the distance between the two spirals when parallel to each other, and that the induced current in B is proportional to the number of reversals of the battery current passing through spiral A, and also to the strength of the current so passing.

Faraday's fertile imagination would naturally suggest the question, "Is this lateral action, which we call magnetism, extended to a distance by the action of intermediate particles?" If so, then it is reasonable to expect that all substances would not be affected in the same way, and therefore different results would be obtained if different media were interposed between the inductor and what I will merely call for distinction the inductometer.

With a view to proving this experimentally, Faraday constructed three flat helices, and placed them parallel to each other, a convenient distance apart. The middle helix was so arranged that a voltaic current could be sent through it at pleasure. A differential galvanometer was connected with the other helices in such a manner that when a voltaic current was sent through the middle helix its inductive action on the lateral helices should cause currents in them, having contrary directions in the coils of the galvanometer. This was a very prettily-arranged electric balance, and by placing plates of different substances between the inductor and one of the inductometers, Faraday expected to see the balance destroyed to an extent which would be indicated by the deflection of the needle of the galvanometer. To his surprise he found it made not the least difference whether the intervening space was occupied by such insulating bodies as air, sulphur, and shellac, or such conducting bodies as copper and the other non-magnetic metals. These results, however, did not satisfy him, as he was convinced that the interposition of the non-magnetic metals, especially of copper, did have an effect, but that his apparatus was not suitable for making it visible. It is to be regretted that so sound a reasoner and so careful an experimenter had not the great advantage of the assistance of such suitable instruments for this class of research as the mirror galvanometer and the telephone. But although he could not practically demonstrate the effects which by him could be so clearly seen, it redounds to his credit that, as the improvement in instruments for this kind of research has advanced, the results he sought for have been found in the direction in which he predicted.

A and B will now be placed a definite distance apart, and comparatively slow reversals from ten Leclanché cells sent through spiral A; you will observe the amount of the induced current in B as shown on the scale of the galvanometer in circuit with this spiral. Now midway between the two spirals will be placed a plate of iron, as shown in Plate 2, and at once you observe the deflection of the galvanometer is reduced by less than one-half.

showing clearly that the presence of the iron plate is in some way influencing the previous effects. The iron will now be removed, but the spirals left in the same position as before, and by increasing the speed of the reversals you see a higher deflection is given on the galvanometer. Now, on again interposing the iron plate, the deflection falls to a little less than one-half, as before. I wish this fact to be carefully noted.

The experiment will be repeated with a plate of copper of precisely the same dimensions as the iron plate, and you observe that, although the conditions are exactly alike in both cases, the interposition of the copper plate has apparently no effect at the present speed of the reversals, although the interposition of the iron plate under the same conditions reduced the deflection about 50 per cent. We will now remove the copper plate, as we did the iron one, and increase the speed of the reversals to the same as in the experiment with the iron, and you observe the deflection on the galvanometer is about the same as it was on that occasion. Now, by replacing the copper plate to its former position, you will note how rapidly the deflection falls. We will now repeat the experiment with a plate of lead, and you will see that, like the copper, it is unaffected at the low speed, but there the resemblance ceases, for at the high speed it has but very slight effect. Thus these metals, iron, copper, and lead, appear to differ as widely in their electrical, as they do in their mechanical properties. Of course it would be impossible to obtain accurate measurements on an occasion like the present, but careful and reliable measurements have been made, the results of which are shown on the sheet before you marked 3. It will be seen by reference to these

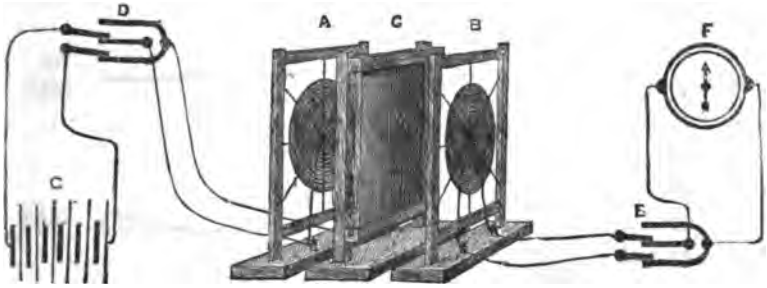


Plate 2.

results that the percentage of inductive energy intercepted does not increase for different speeds of the reverser in the same rate with different metals, the increase with iron being very slight, while with tin it is comparatively enormous. It was observed that time was an important element to be taken into account while testing the above metals—that is to say, the lines of force took an appreciable time to polarise the particles of the metal placed in their path, but having accomplished this they passed more freely through it.

Now let us go more minutely into the subject by the aid of Plate 4, Figs. 1 and 2. In Fig. 1 let A and B represent two flat spirals, spiral A being connected to a battery with a key in circuit, and spiral B connected to a galvanometer; then, on closing the battery circuit, an instantaneous current is induced in spiral B. If a non-magnetic metal plate half an inch thick be placed midway between the spirals, and the experiment repeated, it will be found that the induced current received by B is the same in amount as in the first case. This does not prove, as would at first appear, that the metal plate fails to intercept the inductive radiant energy, and it can scarcely be so; for if the plate is replaced by a coil of wire, it is found that induced currents are set up therein, and therefore inductive radiant energy must have been intercepted. This apparent contradiction may be explained as follows:—In

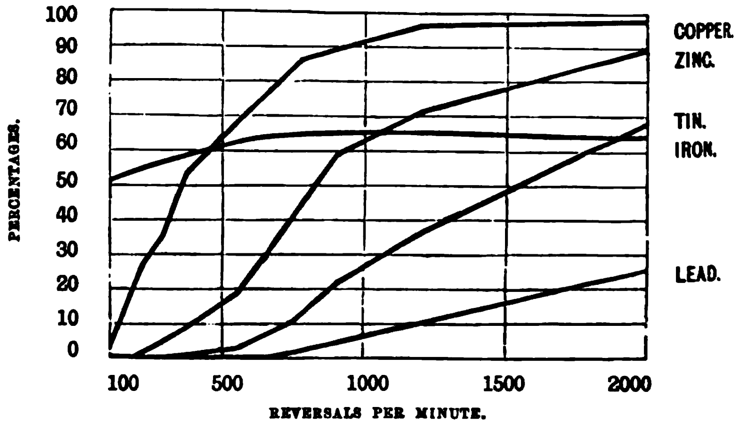


PLATE 3.

Fig. 2 let D represent a source of heat (a vessel of boiling water for instance), and E a sensitive thermometer receiving and measuring the radiant heat. Now if, for instance, a plate of vulcanite is interposed, it cuts off and absorbs a part of the radiant heat emitted by D, and thus a fall is produced in the thermometer reading. But the vulcanite, soon becoming heated by the radiant heat cut off and absorbed by itself, radiates that heat, and causes the thermometer reading to return to about its original amount.

Fig. 1.

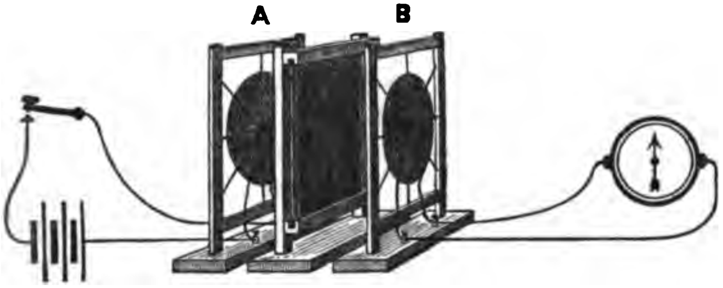


Fig. 2.

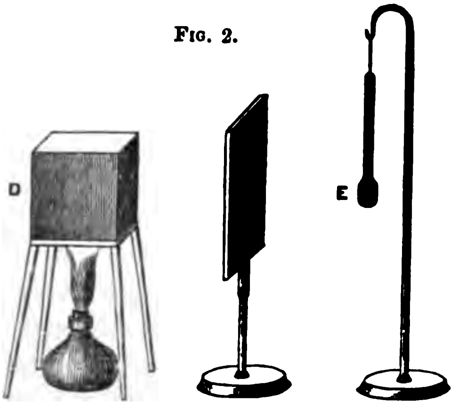


Plate 4.

The false impression is thus produced that the original radiated heat was unaffected by the vulcanite plate; instead of which, as a matter of fact, the vulcanite plate had cut off the radiant heat, becoming heated itself in so doing, and was consequently then the radiating body affecting the thermometer.

The effect is similar, in the case of induction, between the two spirals. Spiral A induces and spiral B receives the induced effect.

The metal plate being then interposed cuts off and absorbs either all or part of the inductive radiant energy emitted by A. The inductive radiant energy thus cut off, however, is not lost, but is converted into electrical energy in the metal plate, thereby causing it to become, as in the case of the vulcanite in the heat experiment, a source of radiation which compensates, as far as spiral B is concerned, for the original inductive radiant energy cut off. The only material difference noticeable in the two experiments is, that in the case of heat the time that elapses between the momentary fall in the thermometer reading (due to the interception by the vulcanite plate of the radiant heat) and the subsequent rise (due to the interposing plate itself radiating that heat) is long enough to render the effect clearly manifest; whereas in the case of induction, the time that elapses is so exceedingly short that, unless special precautions are taken, the radiant energy emitted by the metal plate is liable to be mistaken for the primary energy emitted by the inducing spiral.

The current induced in the receiving spiral by the inducing one is practically instantaneous; but on the interposition of a metal plate the induced current, which, as before described, is set up by the plate itself, has a perceptible duration depending upon the nature and mass of metal thus interposed. Copper and zinc produce in this manner an induced current of greater length than metals of lower conductivity, with the exception of iron, which gives an induced current of extremely short duration. It will therefore be seen that, in endeavouring to ascertain what I term the specific inductive resistance of different metals by the means described, notice must be taken of, and allowance made for, two points. Firstly, that the metal plate not only cuts off, but itself radiates; and secondly, that the duration of the induced currents radiated by the plates varies with each different metal under experiment.

This explains the fact before pointed out, that the apparent percentage of inductive radiant energy intercepted by metal plates varies with the speed of the reversals; for in the case of copper the induced current set up by such a plate has so long a duration that, if the speed of the reverser is at all rapid, the induced current has not time to exhaust itself before the galvanometer is reversed, and thus the current, being on the opposite side of the galvanometer, tends to produce a lower deflection. If the speed of the reverser be further increased, the greater part of the induced current is received on the opposite terminal of the galvanometer, so that a negative result is obtained.

We know that it was the strong analogies which exist between electricity and magnetism that led experimentalists to seek for proofs that would identify them as one and the same thing, and it was the result of Professor Oersted's experiment, to which I have already referred, that first identified them.

Probably the time is not far distant when it will be possible to demonstrate clearly that heat and electricity are as closely allied; then, knowing the great analogies existing between heat and light, may we not find that heat, light, and electricity are modifications of the same force or property, susceptible under varying conditions of producing the phenomena now designated by those terms? For instance, friction will first produce electricity, then heat, and lastly light.

As is well known, heat and light are reflected by metals; I was therefore anxious to learn whether electricity could be reflected in the same way. In order to ascertain this, spiral B was placed in this position, which you will observe is parallel to the lines of force emitted by spiral A. In this position no induced current is set up therein, so the galvanometer is not affected; but when this plate of metal is placed at this angle it intercepts the lines of force, which cause it to radiate, and the secondary lines of force are intercepted and converted into induced currents by spiral B to the power indicated by the galvanometer. Thus the phenomenon of reflection appears to be produced in a somewhat similar manner to reflection of heat and light. The whole arrangement of this experiment is as shown on the sheet before you numbered 5, which I need not, I think, more fully explain to you than by saying that the secondary lines of force are represented by the dotted lines.

Supported in this wooden frame marked C is a spiral similar in construction to the one marked B, but in this case the copper wire is ·044 inch in diameter, silk-covered, and consists of 365 turns, with a total length of 605 yards: its resistance is 10·2 ohms: the whole is enclosed between two thick sheets of card-paper. The two ends of the spiral are attached to two terminals placed one on either side of the frame; a wire from one of the terminals is connected to one pole of a battery of 25 Leclanché cells, the other pole being connected with one terminal of a reverser, the second terminal of which is connected to the other terminal of the spiral.

Now if this very small spiral which is in circuit with the galvanometer and a reverser be placed parallel to the centre of spiral C, a very large deflection will be seen on the galvanometer scale; this will gradually diminish as the smaller spiral is passed slowly over the face of the larger, until on nearing the edge of the latter the smaller spiral will cease to be affected by the inductive lines of force from spiral C, and consequently the galvanometer indicates no deflection. But if this smaller spiral be placed at a different angle to the larger one, it is, as you observe by the deflection of the galvanometer, again affected. This experiment is analogous to the one illustrated by Diagram 6, which represents the result of an experiment made to ascertain the relative strength or capability of producing inductive effects of different parts of a straight electro-magnet.

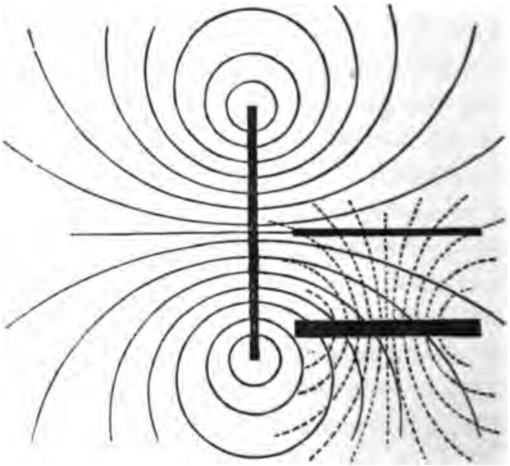


Plate 5.

C, Fig. 1, represents the iron core; PP the primary coil, connected at pleasure to one Grove cell, B, by means of the key, K; S a small secondary coil free to move along the primary coil while in circuit with the galvanometer, G. The relative strength of any particular spot can be obtained by moving the coil S exactly over the required position. The small secondary coil is only cut at right angles when it is placed in the centre of the magnet, and as it is moved towards either pole so the lines of force cut it more and more obliquely. From this it would appear that the results obtained are not purely dependent upon the strength of that portion of the magnet over which the secondary coil is placed, but principally upon the angle at which the lines of force cut the coil so placed. It does not follow, therefore, that the centre of the magnet is its strongest part, as the results of the experiments at first sight appear to show.

FIG·1

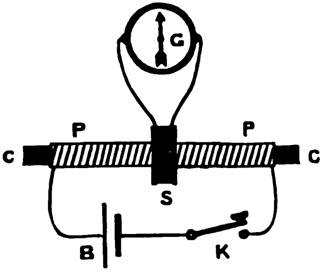


FIG.2

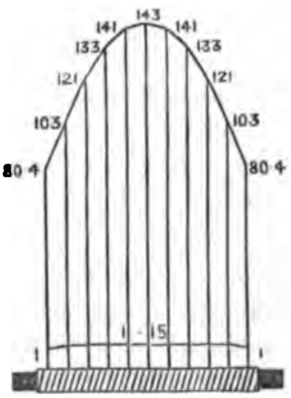


FIG.3

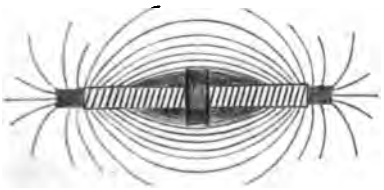


Plate 6.

It was while engaged on these experiments that I discovered that a telephone was affected when not in any way connected with the spiral, but simply placed so that the lines of force proceeding from the spiral impinged upon the iron diaphragm of the telephone. Please to bear in mind that the direction of the lines of force emitted from the spiral is such that, starting from any point on one of its faces, a circle is described extending to a similar point on the opposite side. The diameter of the circles described decreases from infinity as the points from which they start recede from the centre towards the circumference. From points near the circumference the circles or curves are very small. To illustrate this to you, the reverser now in circuit with spiral C will be replaced by a simple make-and-break arrangement, consisting of a small electro-magnet fixed between the prongs of a tuning-fork, and so connected that the electro-magnet influences the arms of the fork, causing them to vibrate to a certain pitch. The apparatus is placed in a distant room to prevent the sound being heard here, as I wish to make it inductively audible to you. For that purpose I have here a light spiral which is in circuit with this telephone. Now, by placing the spiral in front of spiral C, the telephone reproduces the sound given out by the tuning-fork so loudly, that I have no doubt all of you can hear it. Here is another spiral, similar in every respect to spiral C; this is in circuit with a battery and an ordinary mechanical make-and-break arrangement, the sound given off by which I will now make audible to you in the same way that I did the sound of the tuning-fork. Now you hear it. I will change from the one spiral to the other several times, as I want to make you acquainted with the sounds of both, so that you will have no difficulty in distinguishing them the one from the other.

There are suspended in this room self-luminous bodies which enable us by their rays or lines of force to see the non-luminous bodies with which we are surrounded. There are also radiating in all directions from me, while speaking, lines of force or sound waves which affect more or less each one of you. But there are also in addition to, and quite independent of the lines of force just mentioned, magnetic lines of force which are too subtle to be recognised by human beings, consequently, figuratively, we are both blind and deaf to them. However, they can be made manifest either by their action on a suspended magnet or on a conducting body moving across them; the former showing its results by attraction and repulsion, the latter by the production of an electric current. For instance, by connecting the small flat spiral of copper wire in direct circuit with the galvanometer, you will perceive that the slightest movement of the spiral generates a current of sufficient strength to very sensibly affect the galvanometer; and, as you observe, the amplitude of the deflection depends upon the speed and direction in which the spiral is moved. We know that by moving a conductor of electricity in a magnetic field we are able to produce an electric current of sufficient intensity to produce light resembling in all its phases that of solar light; but to produce these strong currents very powerful artificial magnetic fields have to be generated, and the conductor has to be moved therein at a great expenditure of heat energy. May not the time arrive when we shall no longer require these artificial and costly means, but have learned how to adopt those forces of nature which we now so much neglect. One ampère of current passing through an ordinary incandescent lamp will produce a light equal to ten candles, and I have shown that, by simply moving this small flat spiral, a current is induced in it from the earth's magnetic field equal to ·0007 ampère. With these facts before us, surely it would not be boldness to predict that a time may arrive when the energy of the wind or tide will be employed to produce, from the magnetic lines of force given out by the earth's magnetism, electrical currents far surpassing anything we have yet seen or of which we have heard. Therefore let us not despise the smallness of the force, but rather consider it an element of power from which might arise conditions far higher in degree, and which we might not recognise as the same as this developed in its incipient stage.

If the galvanometer be replaced by a telephone, no matter how the spiral be moved, no sound will be heard, simply because the induced currents produced consist of comparatively slow undulations, and not of sharp variations suitable for a telephone. But by placing in circuit this mechanical make-and-break arrangement the interruptions of the current are at once audible, and by regulating the movement of the spiral I can send signals which, if they had been pre-arranged, might have enabled us to communicate intelligence to each other by means of the earth's magnetism. I show this experiment more with a view to illustrate the fact that for experiments on induction both instruments are necessary, as each makes manifest those currents adapted to itself.

The lines of force of light, heat, and sound can be artificially produced and intensified, and the more intense they are the more we perceive their effects on our eyes, ears, or bodies. But it is not so with the lines of magnetic force, for, it matters not how much their power is increased, they appear in no way to affect us. Their presence can, however, be made manifest to our eyes or ears by mechanical appliances. I have already shown you how this can be done by means of either a galvanometer or a telephone in circuit with a spiral wire.

I have already stated that while engaged in these experiments I found that, as far as the telephone was concerned, it was immaterial whether it was in circuit with a spiral or not, as in either case it accurately reproduced the same sounds; therefore, much in the same way as lenses assist the sight or tubes the hearing, so does the telephone make manifest the lines of intermittent inductive energy. This was quite a new phenomenon to me, and on further investigation of the subject I found that it was not necessary to have even a telephone, for by simply holding a piece of iron to my ear and placing it close to the centre of the spiral I could distinctly hear the same sounds as with the telephone, although not so loud. The intensity of the sound was greatly increased when the iron was placed in a magnetic field. Here is a small disc of iron similar to those used in telephones, firmly secured in this brass frame; this is a small permanent bar magnet, the marked end of which is fixed very closely to, but not touching, the centre of the iron disc. Now, by applying the disc to my ear, I can hear the same sounds that were audible to all of you when the telephone in circuit with a small spiral was placed in front of and close to the large spiral; to me the sound is quite as loud as when you heard it; but now you are one and all totally deaf to it. My original object in constructing two large spirals was to ascertain whether the inductive lines of force given out from one source would in any way interfere with those proceeding from another source. By the aid of this simple iron disc and magnet it can be ascertained that they do in no way interfere with each other, therefore the direction of the lines proceeding from each spiral can be distinctly traced; for when the two spirals are placed parallel to each other at a distance of three feet apart, and connected to independent batteries and transmitters, as shown in Plate 7, each transmitter having a sound perfectly distinct from that of the other, when the circuits are completed the separate sounds given out by the two transmitters can be distinctly heard at the same time by the aid of a telephone, but by placing the telephone in a position neutral to one of the spirals, then only the sound proceeding from the other can be heard. These results occur in whatever position the spirals are placed relatively to each other, thus proving that there is no interference with or blending of the separate lines of force. The whole arrangement will be left in working order at the close of the meeting for any gentlemen

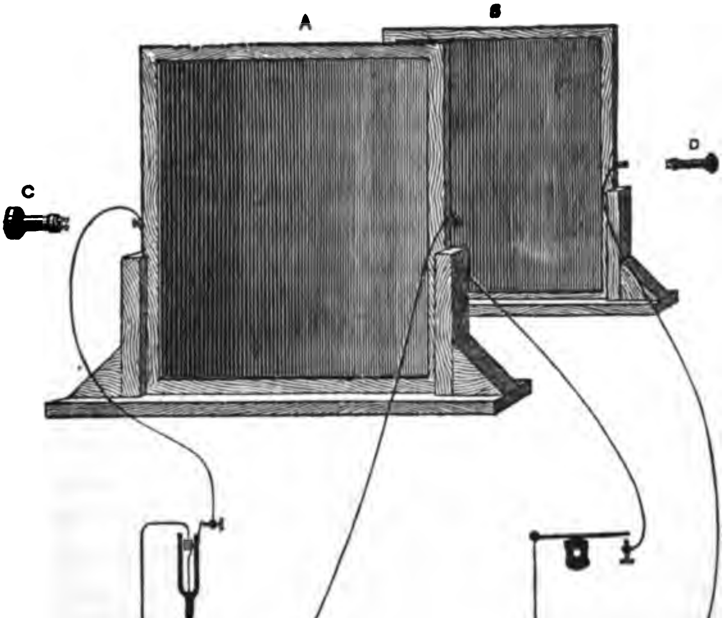


PLATE 7.

present to verify my statements or to make what experiments they please.

In conclusion, I would ask, What can we as practical men gather from these experiments? A great deal has been written and said as to the best means to secure conductors carrying currents of very low tension, such as telephone circuits, from being influenced by induction from conductors in their immediate vicinity employed in carrying currents of comparatively very high tension, such as the ordinary telegraph wires. Covering the insulated wires with one or other of the various metals has not only been suggested, but said to have been actually employed with marked success. Now it will be found that a thin sheet of any known metal will in no appreciable way interrupt the inductive lines of force passing between two flat spirals; that being so, it is difficult to understand how inductive effects are influenced by a metal covering as described.

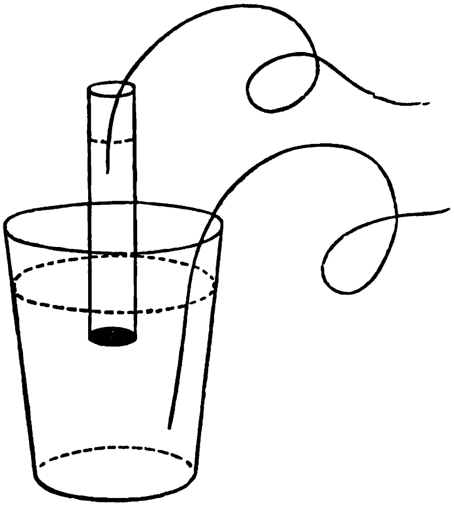
Telegraph-engineers and electricians have done much towards accomplishing the successful working of our present railway system, but still there is much scope for improvements in the signalling arrangements. In foggy weather the system now adopted is comparatively useless, and recourse has to be had at such times to the dangerous and somewhat clumsy method of signalling by means of detonating charges placed upon the rails. Now it has occurred to me that volta induction might be employed with advantage in various ways for signalling purposes. For example, one or more wire spirals could be fixed between the rails at any convenient distance from the signalling station, so that, when necessary, intermittent currents could be sent through the spirals; and another spiral could be fixed beneath the engine or guard's van and connected to one or more telephones placed near those in charge of the train. Then as the train passed over the fixed spiral, the sound given out by the transmitter would be loudly reproduced by the telephone, and indicate by its character the signal intended. One of my experiments in this direction will perhaps better illustrate my meaning. The large spiral was connected in circuit with twelve Leclanché cells and the two make-and-break transmitters before described. They were at connected that either transmitter could be switched into circuit when required, and this I considered the signalling station. This small spiral was so arranged that it passed in front of the large one at the distance of eight inches, and at a speed of twenty-eight miles per hour. The terminals of the small spiral were connected to a telephone fixed in a distant room, the result being that the sound reproduced from either transmitter could be clearly heard and recognised every time the spirals passed each other. With a knowledge of this fact I think it will be readily understood how a cheap and efficient adjunct to the present system of railway signalling could be obtained by such means as I have ventured to bring to your notice this evening.

Thus have I given you some of the thoughts and experiments which have occupied my attention during my leisure. I have been long under the impression that there is a feeling in the minds of many that we are already in a position to give an answer to almost every question relating to electricity or magnetism. All I can say is, that the more I endeavour to advance in a knowledge of these subjects, the more am I convinced of the fallacy of such a position. There is much yet to be learnt, and if there be present either member, associate, or student to whom I have imparted the smallest instruction, I shall feel that I have not unprofitably occupied my time this evening.

Professor W. G. **ADAMS**: The hearty way in which you have received the paper of our President leaves very little for me to say in the way of recommending it to you; but I would ask you formally to ratify what you have already expressed, by giving him a vote of thanks for the very able paper which he has brought before us. It was extremely interesting, and I am sure that we all feel that we have been very profitably employed in listening to it. I hope that it will provoke a very good discussion, seeing that it touched upon so many interesting points which many of our members present will no doubt like to discuss, and we have time this evening for the discussion. The subject of induction is full of interest, and one which we have frequently had brought before us. Among the most prominent of those who have studied and applied measurement to the subject stands Professor Hughes, who is accustomed to weigh all those inductive actions in his balance, and he will no doubt give us a few ideas in opening the discussion.

Professor D. E. HUGHES: I should have been most happy to have followed your advice, but I shall not be able to give you very much, because I have not seen the paper before this instant. I must say that it is a very valuable paper, and I should prefer giving it a little study before attempting to fully discuss it. Some of the experiments brought before us to-night I recognise as some effects that I have studied by aid of the induction-balance. Mr. Willoughby Smith has presented the effects in an entirely new light, and has brought before us a new theory to bear upon them, and I must say that some thought is necessary before they can be properly considered. The introduction of a sheet of copper between two coils has the effect of delaying or weakening the induced current: it absorbs work, but it is impossible in such a case to obtain a balance by simply moving the coils away. If that was not the case, anything could be measured without trouble by simply moving the coils, but the question of time comes in, and makes the question very difficult of adjustment. Each metal has a peculiar time of discharge, but in all non-magnetic bodies a retardation takes place. I would like to point out to Mr. Willoughby Smith that if I make a spiral of copper and leave the two ends open there is not the slightest effect in the induction-balance, but if I close the ends then the full effect is perceived. Some of the most valuable points that Mr. Willoughby Smith has brought out seems to me due to the difference in time, which is very important. He has noticed that with a different time of contact there is a different effect. That is a very important point, because in an induction-balance we can and do measure the conductibility of metals, but we do not measure it as electricians ordinarily measure it: they measure when electricity is in a state of flowing, or stable condition; in induction we measure the first portion of flow, or its period variable. Now, from the period variable to the period stable we have a very rapid curve, the form and height of which varies with each metal. I must confess that in the induction-balance I could not trace this difference with any method of accurately proving such to be the case, and Mr. Willoughby Smith's results will be a most valuable point to bring forward.

Mr. A. J. S. **Adams**: I should like, before the discussion proceeds, to ask an expression of opinion upon the action of the atmosphere in connection with induction experiments, because to my mind the importance of the question has not been sufficiently recognised. To illustrate my point, permit me to quote the following experiment:—

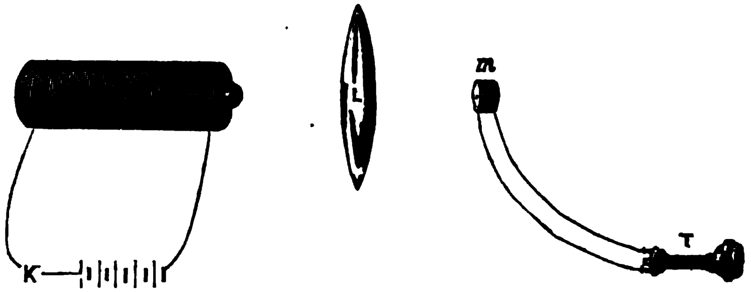


The sketch represents a glass tumbler containing some water, immersed in which is one end of the secondary of an induction-coil. Dipping into the water is also a glass tube having its lower end closed by a piece of waxed paper; the tube contains water, and into it dips the other end of the secondary. The two liquids are therefore divided by the thin diaphragm at the bottom of the tube, but although the induction-coil armature be caused to emit a musical note, the arrangement as it stands will not reproduce it. If the single diaphragm be now replaced by a double, so that the lower end of the tube is closed by two thin discs of insulating material with an air film between them, the musical note will be clearly reproduced in the tumbler, and the question arises, what part does the air really play?

That the atmospheric air does exert an important and possibly a primary influence in such a case was particulary brought under notice—and, I think, for the first time—in a paper read before this Society on December 12th, 1877, and printed in the Journal 20 for that year.

Mr. W. H. **Preece**: Mr. Chairman,—I am very much in the same position as Professor Hughes—that is, I have not had an opportunity of reading or studying this paper. It is a paper that has been prepared with great skill. Mr. Willoughby Smith himself has said that it has occupied his attention since the last time we were in this room, and there is no doubt that that time has been very well spent. It has been spent in devising a series of experiments that must have conveyed conviction to every one of us in this room. Now these effects of induction have really occupied the attention of practical telegraphists only since the introduction of the telephone. But before even the telephone was introduced it was known that they existed, and it was known that they must produce some results on our telegraphs. One very striking instance of these effects was brought to my notice in the following curious way:—On one occasion when I was giving a lecture at the Albert Hall, a wire was brought to that hall in connection with India, against my will and against my wish. Now I am an old hand at lecturing, and have also had something to do with the getting up of soirées, and have more than once been the victim of accidental or other "hankey-pankeyism," and I was quite certain that if a wire was brought to the Albert Hall there would be some failure. On one occasion, the first lecture I ever gave in my life,—it was in Southampton,—I had a wire in connection with London, and arrangements were made so that I might communicate with the Continent by the new cable in the North Sea; and when that part of my lecture was reached where we were to be placed in connection with the Continent, we first of all spoke to Amsterdam, and asked how the weather was, and the answer came, saying, "It is very wet." We were then placed in communication with Berlin, and the same question was asked, and then we went in communication with Vienna. In the early part of the lecture, I had described the effect of the difference in time due to difference of longitude—how it was that messages received in London were timed an hour later from Vienna; and when we were put in communication with Vienna an inquisitive editor amongst my audience said, "Now let us test what Mr. Preece told us about time. What is the time in Vienna?" Of course I had the question put to Vienna, the time being nine o'clock in Southampton, and the answer came back that it was "eight o'clock." The editor said, "That's a lie! the time in Vienna is nearer half-past ten;" and on enquiry it was found that my friend in London, instead of putting us in communication with the Continent, had personated the different stations and answered the questions himself. He knew that the difference of time was about an hour, but instead of putting it on he took it off. I was as much victimised as my audience, and that has always made me rather suspicious of through communications; and so at the Albert Hall, when I was told that we were going to speak to India, I said nothing. However, there was the wire, and there were messages sent to India. We were told that "the locusts were swarming in Scinde." But it happened that at the selfsame hour, while I was lecturing in the Albert Hall, my brother, who was stationed at Shiraz, in Persia, was knocked up—at to him about three or four o'clock in the morning—by one of the clerks, who said that there was something on the wire going on about him. So he got out of bed, and found that on the wire at Shiraz there was a message going through from the Albert Hall to India, and that this message was about the locusts in Scinde, and he sent me the slip itself as a proof that the message went. Now, the direct wire through Persia to Kurrachee ran side by side with the local circuit upon which was Shiraz, and this message about the locusts at Scinde was read at Shiraz by induction. The currents passing over the through wire induced currents in the local wire, and so produced the message, which convinced me that on that occasion there was no "hankey-pankeyism" at all in the Albert Hall arrangements. Another way in which these inductive effects act is by retarding messages. Where you have two wires running side by side for some distance, the same effect that we saw to-night is repeated on the second wire, and the effect is retardation; but when the telephone came in, then these effects of induction began to have practical significance, and we heard in this room how Professor Graham Bell had set intermittent currents working in the cellar of his house in Washington, and heard the induced currents on the very top of the house. There is not any one in this room who has used the telephone who has not experienced these inductive effects. Like Mr. Willoughby Smith, we have endeavoured to shut off these effects of induction with screens, and it has been said by some people, as also in Mr. Willoughby Smith's paper, that the screens have failed or have only partially cut off the effects. Now in all telephone circuits we have three great sources of disturbance: we have this effect of induction, we have the effect of leakage, and we have the disturbance due to the currents that are constantly percolating through the earth—not only earth currents, but currents from neighbouring telephone and telegraph stations. The effect of screens in the way of an outside coating of lead, iron, or brass has been, not to screen induction at all, as Mr. Willoughby Smith shows, but to remove the earth and the leakage currents. From our experience in the Post Office, no insertion of plates or screens will prevent this induction between wire and wire; and a great deal of money is at the present moment being uselessly expended in trying to stem the influence of induction by the use of these screens. Now Mr. Willoughby Smith has not only to-night brought before us very clearly and very ably these effects, but he certainly has pointed out a very interesting fact. It is said that when a man is able to make two blades of grass grow where one blade grew before, he is a benefactor of mankind; and when he has added to our practical knowledge, he is a benefactor to the Society of Telegraph-Engineers. Mr. Willoughby Smith certainly has shown us to-night that time plays a function in these experiments that was not, as far as I remember, known before, and therefore he has added to our store of knowledge. But he has put before us another fact that I in another way have once or twice endeavoured to put before you, and that is, we must regard these electrical phenomena as molecular phenomena—that all these illustrations of electrical action are merely in form and in character very similar to those undulatory phenomena of light and heat. All the tendency of teaching at the present day, both among physicists and amongst practical men, is to show that the effects of electricity are somewhat similar to those of light and heat, and that they are to be attributed to some peculiar vibratory motion of the ultimate particles of matter; and the more the subject is studied the more this is sure to come out. Professor Hughes is working with great ardour in this direction, and others are working in the same field, and day by day our knowledge is increasing and we are getting to know more and more of what this subtle and delicate form of energy is. I am sure we are all very much indebted to Mr. Willoughby Smith for what he has brought before us, but I should like him to have alluded to the labours of a gentleman who has worked in this direction (who does not belong to our Society, I think, but who ought to), and that is Dr. Tribe. Dr. Tribe's papers read before the Royal Society are most interesting. He has reproduced phenomena in which he shows that electricity, like light and heat, produces phenomena very analogous to those of refraction. Now when it is proved that electricity, like light and heat, can be reflected and refracted, then I think nothing more will remain to convince everybody that electricity is nothing but mere motion of molecules of matter. I did not follow Mr. Adams in his experiment, but this I do know, that whether the air be present or not—whether it be through a vacuum or not—whether it be through conductors or insulators, through gases or liquids, this inductive action produces exactly the same effects, and therefore it must be an effect of the ether that fills all space. All these phenomena not only show that they must be attributable to the undulations of the ether, but they also show that there must be an ether; and while there are many who study the phenomena of light and the various phenomena, endeavouring to prove that it all can be done without the existence of ether, these experiments which we have seen to-night, and analogous ones, show that an ether must of necessity pervade all space. The void from the earth to the sun, from the planets to ourselves, is filled with this light and delicate mass that enables us to see with our eyes, to feel with our bodies, and we know also that it acts upon our magnets and our telegraph wires. There are phenomena of earth currents which follow the spots of the sun; there are changes in the magnetism of the earth which show unmistakably that there is a transference of energy between the earth and the sun that can only be conveyed by this ether. Now, one word more. There is an old Latin proverb, *Ne sutor ultra crepidam:* it means that a cobbler should stick to his last; and I almost wish that Mr. Willoughby Smith had stuck to his last, but he departed from the field in which he is a master and entered upon that in which he has something yet to learn—I mean the working of trains on our railways. He quotes an experiment, but I doubt very much whether the use of a telephone in the hands of an engine-driver would enable him to learn the condition of his distant signals with such accuracy as is now imparted to him by the fog-signal. When thick weather comes on, an engine-driver cannot see ten feet in front of him: he only wants to see that distance to distinguish his signals. It would be absurd and ridiculous to use a really beautiful practical instrument like the telephone to convey to him information of things that exist only ten feet off. The present practice of giving warning to an engine-driver in thick fogs is to place a man at each distant signal, who puts one detonating signal on the metals to mean that all is clear, two detonating signals to intimate caution, and three detonating signals to bring the train to a standstill, and it would be quite impossible to communicate such information so accurately and surely by telephone as by the present practice.

Professor W. E. Ayrton: In connection with the interesting experiment which Mr. Willoughby Smith has been so good as to bring before us, I should like to refer to the analogy with heat and light. A very pretty experiment was shown us where two coils were placed at right angles to one another, and no effect of induction was produced; but on putting a certain plate of metal between the coils, an effect was observed, and the effect was compared with the effect of reflection. I think Mr. Smith is possibly a little mistaken in using the word *reflection* there. No doubt an effect is produced by the insertion of the plate of metal, but it is not at all produced by reflection, or anything in my opinion analogous with reflection. I can say, speaking generally, so far as I am aware no experiment has ever been shown where there is what you may call reflection, or refraction, of electricity. The essence of a plate reflecting a ray of light is that it does not absorb any of the energy—that it is not self-luminous. If the body or plate absorbs the light and gives it out again, of course the word reflection may be used, but it is not quite a suitable one. The plate becomes self-luminous, and that is what takes place when you are dealing with the two coils placed at right angles to one another. The plate itself acts like one of the induction coils, and absorbs a certain amount of the energy; it is in fact exactly like an opaque substance having rays of heat falling on it, becoming warm and radiating heat in all directions—not in one direction particularly, but in all directions; but that is not what is meant by metallic reflection. So in the same way certain experiments may be made which at first sight may look like *refraction*, but it is not *bonâ fide* refraction. About two years and a half ago, when my colleague and myself were determining electrically the index of refraction of ebonite for light, we made certain other experiments, and obtained results which looked uncommonly like effects produced by refraction of electricity through a solid copper lens, for we obtained an electric focus, but this focus, however, was not really a focus produced by refraction. The experiment was very simple, and as follows:—M was a powerful electro-magnet, the current flowing round which was rapidly made and broken by an automatic key at K. L was a solid copper lens, and we searched for a focus, not of course with the eye, but with a small exploring coil, *m*, the two ends of which



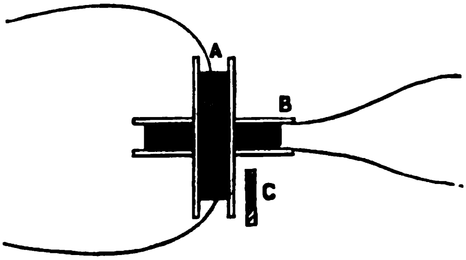
were attached to a *distant* telephone, T. It was very difficult to find an exact focus, owing to the smallness of the action, but there was found a small region, such that if the exploring coil, *m*, was within it, the sound was louder than if *m* were without it.

But the point I particularly want to draw attention to is this—that although an effect is produced analogous with a focus, it is not produced by refraction. Every particle of the copper, L, as Professor Hughes has rightly pointed out, has induced currents set up in it which produce inductive effects on the exploring coil, *m*, and you obtained simply the resultant effect of the copper lens, L, which becomes, so to say, electrically self-luminous. It is not that rays of radiant energy, or whatever they may be called, are passing through and being bent, as the rays of light or heat are, without being themselves absorbed by the lens, but the energy is distinctly absorbed by the copper lens, and sent out again in the same way that a hot body sends out heat. Although, then, this looks at first sight very much like refraction, it is essentially not refraction; and I may add that, mentioning this experiment to Sir William Thomson at the time, he was indignant at my even using the word refraction in connection with electricity, although of course I was merely using it as an illustration of the apparent effect. Pretty experiments can be made with results which seem to be analogous with refraction and reflection, but, to sum up what I have been saying, I know of no experiment which really proves that electricity or electric rays can be reflected or refracted. There is one point in Mr. Willoughby Smith's interesting paper which caught my eye, and which doubtless is a misprint. The paper reads that the effect of one coil on the other is "inversely as the square of the distance." Of course that would not be the case, and I take the liberty of drawing Mr. Willoughby Smith's attention to what doubtless is a clerical error. If two coils of the same dimensions, such as Mr. Willoughby Smith is employing, be used for the primary and secondary, and if they be put parallel to one another at various distances apart, reckoned from their centres, then I find from theoretical considerations that for the distances 1, 2, 3, 4, etc., the induced current in the secondary, on making or breaking the current on the primary, are roughly—

Distance.	Actual Strength of Induced Current.	Strength of Induced Current if inversely proportioned to square of distance.
1	22	22
2	18	5½
3	14	2
4	11	8
5	9	—
6	8	—

In a separate column I have added what would be the induced current if it varied inversely as the square of the distance between the coils. The current therefore falls off much more slowly than the inverse square of the distance.

Professor D. E. Hughes: I quite agree with Professor Ayrton about the refraction experiment; and the following sketch will show something that was very similar:—



The above represents one of my first induction-balances, described in *Comptes Rendus*, Dec. 30, 1878, and Jan. 20, 1879, and consists of a primary coil, A, and a secondary coil, B. When these coils were absolutely at right angles, perfect silence was the result. If a piece of metal, C, was introduced, the balance was upset, and if the piece of metal was perfectly at right angles to either coil, the balance was also upset. The effect is very similar to that described by Professor Ayrton, and due to the disturbing effects of the metal upon the lines of force; and, as this resembles somewhat the experiments of Mr. Smith, I thought it sufficiently interesting to mention.

Col. E. D. Malcolm: Gentlemen,—It is a very bold thing to break a lance with Mr. Preece, but with regard to his remarks about the impossibility of using volta-induction for railway working, I would ask him whether we have not often laughed in this room over the reply that was given many years ago when Francis Ronalds proposed to signal by electricity first. It was a new thing, and the Government would not have it. What has been introduced to us to-night is a new thing; I do not say that it is any good, but ought Mr. Preece, a Government official, to condemn it off-hand?

Mr. W. H. PREECE: Allow me to correct myself and Col. Malcolm. I did not say that volta-electric induction will not be useful for railway working. What I did say was, that it could not possibly replace fog-signals as used now for railway signalling.

Mr. C. E. SPAGNOLETTI: I should like to ask one question. In the experiment where Mr. Willoughby Smith showed that heat after a short time affected the thermometer,—that is, that when the screen was taken away the thermometer showed the effect of the heat and its diminution for a time owing to the introduction of the screen,—I would ask whether, if the screen were kept cool by some means when placed between the source of heat and the thermometer, the thermometer would be affected? One would think not. If that were so, then, looking at the experiment with the coils A B, Fig. 1, Plate 4, if the screen were connected with the earth, would not the connection with the earth prevent or minimise the inductive effect of coil A, upon coil B, and give no effect or reduce the reading upon the galvanometer? As the experiment is illustrated in Fig. 1, Plate 4, there is no connection with the earth at all (which would not be the case, I take it, if the experiment were made at a distance, and the earth used in place of the return wire), and consequently there is nothing to absorb the effects. I would ask whether any difference in the effects would be produced if the earth formed any part of the arrangement? This might be tried by connecting a wire from the metal screen to the return wire from coil A.

The PRESIDENT: You mean that instead of a metallic circuit you have an earth circuit?

Mr. C. E. SPAGNOLETTI: Yes; and attaching a wire from the screen to the earth also, so making the earth form a part of the secondary circuit, between the screen and the A coil circuit.

The PRESIDENT: Yes.

Professor W. G. ADAMS: With regard to the remark which fell from Professor Ayrton on the question of reflection and refraction of inductive action, I would draw attention to Plate 5 in the paper, which suggests the way in which the presence of a plate of metal modifies the form and position of the lines of force. A change of position of the metal produces an alteration in the form of the lines of force, so that, although previously there was no induction current in the second coil, in their altered form the lines of force cut the second coil, and a disturbance or induction current is produced. This would seem to be the explanation suggested by the figure. The induction experiment with the copper lens is explained on the same principle: the metal that is brought into the field modifies that field, and so alters the form of the magnetic lines of force.

There is one part of the paper which stamps it as of great value. Not only has our President told us that time is required for these inductive actions to take place, but he has also laid down the laws of the action of the screens, showing by tables and by the curves in Plate 3 the percentages for different metals for different rates of make and break, or, we may say, different periods of continuance of the primary current. We get at sight, for any particular rate of revolution of the contact-breaker throughout a considerable range, the percentage of inductive action through the different metals. This is a great step in advance, and helps us in establishing the laws of induction currents. I have great pleasure in tendering your thanks to our President for the valuable and interesting communication which he has given us this evening.

The PRESIDENT: Gentlemen,—to be honest with you, I am almost done up—I have a splitting headache; and you must remember that before your Council came into this hall they had had two hours' hard work downstairs managing your business. Therefore those gentlemen who have spoken will perhaps excuse me if I do not reply as fully as I otherwise would.

Professor Adams has just reminded Professor Hughes that I do state in my paper that time is a very important element in measuring what I call the inductive resistance of metals.

I am sorry I did not quite grasp all Mr. Adams said concerning his experiment, and, unfortunately, Professor Ayrton has removed Mr. Adams's diagram to make room for his own. But I would remark that any experiment in which glass cells, water, high-tension currents, and a telephone are used requires more than ordinary care to ensure reliable results.

It is very gratifying to me to hear Mr. Preece admit that I have taught him something this evening. Mr. Preece, however, in referring to my suggestion concerning the adoption of volta-electric induction to railway signalling, says that "every cobbler should stick to his last." I do not quite see the application of the proverb, more especially as the Post Office has not yet taken over the railways. Electrical railway signalling is a subject that I have taken great interest in. Thirty years ago, as a young man aspiring for fame, I exhibited, at the Polytechnic, an electrical railway signalling apparatus. The instrument is still in my possession, and I often look at it with pleasure. Mr. Preece says that, however foggy or dark it may be, railway signals are always distinguishable at a distance of ten yards. All I can say is that I missed my way yesterday in the north of London in a fog in which I should have been thankful if I could have seen a distance of three feet. I hope Mr. Preece or Mr. Spagnoletti, or some gentleman connected with railways, will try the experiment. It would be a very cheap system, and I believe it would answer all that is required. As regards the experiments on induction which Mr. Preece referred to as having been published by Dr. Tribe, I know of no such experiment by that gentleman; but if Mr. Preece will kindly inform me where the description is to be obtained, I shall be obliged to him, as I take great interest in all that has been done in induction.

In reply to Professor Ayrton, as regards reflection, I would draw his attention to my paper, in which he will see that I say, "The phenomenon of reflection *appears* to be produced in a somewhat similar manner to the reflection of light and heat." With reference to the "square of the distance," accurate measurements have been made, and I still adhere to my statement, and shall continue to do so until by actual experiment the contrary is proved. I will ask Professor Ayrton if he was aware, when making the electro-magnet experiment he has mentioned, with the lens of copper and the small electro-magnet, that the telephone would be affected by the magnet, as I have shown to-night, quite independently of the coil or copper lens. If he did not then know that fact, I am certain that any experiments he made in that way were not reliable.

Gentlemen, all I can add is, that I have done my best, and am pleased if it has met with your approval.

Professor W. E. AYRTON: One word. I would just mention that the telephone we tried was in a different room altogether from the coil. The exploring coil was moved about, and the telephone was listened to by myself in a different room, so that there was no fear of the telephone being acted upon to any appreciable extent. There was no failure in the experiment, as a focus was actually obtained, but not a focus produced by refraction.

The PRESIDENT: If you will go outside this room with the telephone now before you, and try the experiment with this coil, you will hear the sound of the tuning-fork.

The President then resumed the chair, and announced that the meeting adjourned until November 22nd, when a paper would be read by Messrs. Trott and Hamilton on "Submarine Cables: their Decay and Renewal."

The meeting then adjourned.

The One Hundred and Twenty-sixth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 22nd, 1883—Mr. WILLOUGHBY SMITH, President, in the Chair.

The PRESIDENT: Gentlemen,—At our last meeting I had the painful task to perform of announcing to you the death of one of our esteemed members, a gentleman well known to you all—Mr. Cromwell Fleetwood Varley. To-night I have, if I may say so, a still more painful duty, and that is, to refer to the death of Sir William Siemens, the esteemed first President of this Society. You have lately had before you in the public journals the deeds of that great man, and therefore I will not detain you to-night by going over the same ground. In this Society Sir William Siemens was well known, but I am sure that only those gentlemen who have acted on the Council of this Society can fully know the loss which it has sustained by his decease. Therefore it is proposed that, out of respect to his memory, this meeting adjourn until he is removed to his last resting-place. Previous to doing so, I would suggest that the meeting sanction the following resolution being entered on our minutes:—"That this meeting desires to record its deep sense of the loss the Society has sustained by the decease of its eminent and esteemed first President and friend, Sir William Siemens, and its sincere sympathy with Lady Siemens and the members of the family in their bereavement."

Captain S. TROTT: Mr. President and gentlemen,—Of course I came here to-night with the intention of reading one of the papers announced for this evening. My two friends Mr. Hamilton and Mr. Kingsford, who also intended to take part in this evening's papers, and I, belong to that class of telegraph men who are banished from our homes to a great extent, and we had looked forward to this meeting with pleasure for some time past, so that we might read those papers on Submarine Cables. It is not often that we come to this country, and therefore regret that our intention cannot be carried out, but we more deeply regret the cause. There are many gentlemen present who know that I served the late Sir William Siemens for something like six years, in command of the "Faraday," and I could not this evening think of standing before you to read a paper while his body is lying at a short distance from here, and mourned by those who were dear to him; so I would gladly attach my name to, and second, the motion which our President has proposed.

The President: I am much obliged to Captain Trott for the kind way in which he has spoken of Sir William Siemens, and also for the kind way in which he has foregone the reading of his paper this evening. I now move that this meeting be adjourned until this day week.

Mr. W. T. Ansell seconded the proposition.

The President: That, gentlemen, concludes the business of this evening, and we hope to meet on Thursday next, when the papers set down for to-night will be brought before you.

The meeting then adjourned.

The One Hundred and Twenty-seventh Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 29th November, 1883—Mr. Willoughby Smith, President, in the Chair.

The minutes of the Ordinary General Meetings held on November 8th and 22nd were read and approved.

The names of new candidates were announced.

Donations to the Library were announced as having been received from the following:—The Kinetic Engineering Company; J. J. Storrow, Esq.; Latimer Clark (Past-President); J. Sivewright, C.M.G., Local Hon. Secretary for the Cape; and a hearty vote of thanks was accorded to the donors for their presents.

The following paper was then read:—

TROTT AND KINGSFORD'S AUTOMATIC GRAPNEL FOR SUBMARINE CABLES AND TORPEDO LINES.

Communicated by H. Kingsford, Associate.

All those who are acquainted with the cable-lifting branch of submarine telegraphy are well aware how important a matter it is in grappling to be certain of the instant the cable is hooked. This importance increases, of course, with the age and consequent weakness of the material, as the injury caused by dragging a cable along the bottom is obviously very great.

It is easy also to understand the fact that in nearly all cases the most delicate dynamometers must fail to indicate immediately the presence of the cable on the grapnel, more especially in those cases where a considerable amount of slack grapnel-rope is paid out. In many cases, therefore, the grapnel will travel through a cable without the slightest indication (or at least reliable indication) occurring on the dynamometer, and perhaps several miles beyond the line of cable will be dragged over, either fruitlessly, or to the peril of neighbouring cables; whereas, should the engineer be advised of the cable's presence on the grapnel, the break will probably be avoided and the cable lifted, at any rate the position of the cable will be an assured thing.

My own knowledge of cable grappling has convinced me of these facts; and I am well assured that those engineers at least who have been engaged in grappling for cables in great depths, or for weak cables in shallow water, will heartily agree with me.

In addition to the foregoing remarks *re* the insufficiency of the dynamometer as an instrument for indicating the presence of a cable on the grapnel, I might remind engineers of the troubles and perplexities which occur incessantly in dragging over a rocky bottom. The grapnel hooks a rock, a large increase of strain is indicated on the dynamometer, and it becomes doubtful whether the cable as well is hooked or not. Again, it frequently happens in grappling over a rocky bottom that one or more prongs are broken off, the grapnel thus becoming useless, great waste of time being thus occasioned. Fully realising all the difficulties herein enumerated, it occurred to me that a grapnel might be constructed in such a manner as to automatically signal by electrical means the hooking of the cable, while it would ignore all strain that external causes might bring to bear on it, and thereby obviate the uncertainties attached to the use of the grapnels at present in vogue. To effect this, I designed early in 1881 a grapnel fitted in each prong with an insulated conducting surface, and a plunger and pin so arranged that the cable, when hooked, should by the pressure that it would bring to bear on any of the plungers, cause the pin to come in contact with the conducting surface, itself in electrical communication with any suitable current detecter and battery on board the repairing ship, and thereby complete the circuit. This grapnel was successfully used on the Anglo-American Telegraph Company's repairing steamer "Minia" in the summer of 1881, and a patent, No. 13,665, was granted me in Canada for the same, on November 10th of that year.

Subsequently, in discussing the construction of the grapnel with Captain Trott, we concluded that something was yet wanted to render the successful working in deep water absolutely sure, and we decided, consequently, to make certain alterations, and to secure the improved form of apparatus in England, France, the United States, and Canada. These patents have been granted, and patents have been applied for also in Germany and Belgium over on the bottom, and would be apt to twist and break the wire short off. As a matter of fact, the grapnel will turn, and does turn, with the rope; a swivel is therefore of no value. We are perfectly awake, however, to the fact that a grappling-rope should be made in a manner that will not allow it to kink; and engineers should avail themselves of such rope, especially in deep water. Patents have lately been granted to Messrs. Trott & Hamilton for the invention of a form of rope or cable answering all the requirements of this work.

A small type of grapnel fitted in the manner I have described may be very advantageously used for searching purposes, to ascertain the position either of telegraph or torpedo lines: by towing at a quick rate much time may be saved. The position being ascertained, if it be not desired to lift the cable, the grapnel can be released and hove on board by a tripping line, which can always be attached when such work is contemplated. The great importance of being able to localise an enemy's torpedo lines without raising an alarm will be readily seen by engineers engaged in torpedo work.

References to the Diagrams.

a is the stem of the grapnel containing core.
b, flukes.
c, recess for insulated contact-plate connected to core.
d, covering plate screwed on bottom of grapnel.
e, button of plug.
f, rubber washer and button.
g, metal-plate.
h, stem of plug, on which in the under counter-sink "U" is a sma metal disc which prevents the fittings from falling out.
i, needle.
j, spring.
k, counter-sink for head of plug.
l, counter-sink for spring.

The President: I think, for the purposes of discussion, we may consider the two papers set down for this evening as one, and therefore we will let the discussion follow the reading of the second paper.

The following paper was then read:—

SUBMARINE TELEGRAPH CABLES: THEIR DECAY AND RENEWAL.

By SAMUEL TROTT and FREDERIC ADAM HAMILTON, Associates.

When we consider the enormous extension of ocean telegraphy during the last few years, it is somewhat surprising that so little information has been imparted concerning the condition of cables after various periods of submersion; and, as the subject is both interesting and important, we propose submitting to the Society the result of some of our own experience in connection with the submerging and repairing of submarine lines.

As some ocean cables have now been submerged for considerable periods, and very large sums have to be expended in keeping them alive, we will offer a few remarks on some of the causes of rupture and decay to which these costly properties are liable, and state our views with regard to the measures that should be adopted in order to reduce these serious casualties to a minimum.

It is unnecessary to explain in this paper how submarine cables are manufactured, and of what materials they are composed, seeing that we are all so familiar with, or at any rate have access to, publications bearing on the subject. It is sufficient for us to observe that the leading characteristics with regard to the form or type of cable adopted in the earliest period in the history of submarine telegraphy have been rigidly adhered to up to the present time.

We intend to confine our remarks in this paper to Atlantic cables—first, because they are the most important, and secondly, on account of their being the ones with which we are most familiar.

The principal accidents to which these cables are liable may be briefly enumerated, as follows:—Abrasion by ice and on rocky and stony bottom; ruptures caused by vessels' anchors; injuries inflicted by marine insects; *and the wringing asunder of the core by the iron wires.*

Accidents arising from the first cause have not been numerous; indeed, taking into consideration the heavy masses of ice that beset the coasts and banks of Newfoundland and Cape Breton Island, and extend far west along the shores of Nova Scotia, it is marvellous that so few cases of crushing have occurred.

With regard to breaks on rocky and stony bottom, it is worthy of remark that, excepting in the case of those which have taken place in the immediate vicinity of the shore or in very shallow water, few can be recorded; and we may here state, as the result of long and careful observation, that cables are rarely chafed through, and we believe that injuries attributed to this agency should in some instances be put down to quite another cause.

The number of breaks which have occurred to some cables, in consequence of fishing vessels hooking them with their anchors, is greatly in excess of those brought about by any other means; but as these accidents are to a great extent unavoidable, and are easily and rapidly repaired, we will dismiss this portion of our subject with the remark, that considering the number of vessels engaged in the great fisheries on the banks lying between the meridians of 48° and 70° west, the wonder is that the cables are not broken far more frequently.

In reference to faults caused by marine insects, it can be said that, although the Atlantic cables have not enjoyed a complete immunity, they have been remarkably free from any very serious attacks of these borers, and, as their assaults can be repelled by well-known means, we do not consider the question a very prominent one, as far as Atlantic cables are concerned.

We now have to consider the most serious of all accidents to which submarine cables are liable during the process of submersion, whilst being hove in, and after having been successfully deposited in the depths of the ocean.

All who are familiar with the history of Atlantic cables will remember the very interesting account of the early efforts made to connect the two continents, and how, in spite of difficulties which we will not stop to discuss, these efforts were crowned with success.*

A careful perusal of this detailed account—one of the few concerning such undertakings—will suggest the enquiry, How is it that cables suddenly sever during the process of laying or whilst being picked up, without any abnormal strain being indicated or applied? And why do they, in an equally mysterious manner, break in deep water, after having been successfully submerged?

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* "The Atlantic Telegraph," by W. H. Russell, LL.D.

In the work to which we have alluded, such remarks as the following are of frequent occurrence:—"When suddenly the continuity of the electric current ceased, and the electricians declared that the cable had broken at the bottom. . . . . It snapped close to the ship. . . . . The electricians on board reported that continuity had ceased. . . . . When suddenly it snapped, *without any perceptible cause*, under a strain of only 2,200 pounds. . . . . The strain *one-third less than breaking strain*, everything favourable, and yet the cable parted, *silently and suddenly*." Speaking of one disaster in particular, Dr. Russell says: "The two other breaks might be accounted for and guarded against for the future, but there was something in the latter not so easy of explanation, *and which seemed to point to some mysterious agency existing in the depths of the ocean, beyond the perception of science or man's control*."

The agency described as *mysterious* still exists, and the history of some of the most recent Atlantic cables, if published, would prove that these so-called improved types are liable to the same mishaps as those we have mentioned; indeed, they are even more liable, for notwithstanding their strength, it is quite as difficult, and even more so, to recover them from deep water.

It is greatly to be regretted that records such as Dr. Russell's have not been published respecting other and more recent cable expeditions.

After years of experience in submarine telegraphy, we naturally arrive at conclusions different to those held by some of our predecessors and contemporaries; and although we have no right to attack the past or the present, we are perfectly justified in reviewing both. We should certainly be wanting in ordinary perception if we failed to read between the lines when studying some of our records concerning Atlantic cables.

When we consider the misfortunes which have attended some of the more recent operations in the Atlantic, we feel that the time has come when the question as to the reason for such expensive and disastrous results should be fully discussed. This brings us to the question, Why do cables suddenly sever in deep water whilst being submerged or when being picked up, and even after being successfully submerged?

Iron-armoured cables have a strong tendency to twist or curl, as the spiral *lay* of the wires imparts a constant helical force in a direction contrary to the *lay*.

Consequently, any slack given is apt to result in a kink or a succession of kinks, which is followed by a wringing asunder of the fabric when the strain is renewed. This accounts for most of the misfortunes which have happened in submerging some cables, for whenever it became necessary to stop or slacken speed in shifting from one tank to another, or for some other purpose, the risk of this accident has always been incurred.

An iron-armoured cable cannot be recovered from deep water excepting in short lengths, because the operation of heaving in causes the armour to unlay or untwist, and this results in an accumulation of turns at and near the ocean bed, and a wringing or wrenching asunder of the fabric is sure to occur.

Cables sometimes break in deep water, because when the iron wires become weakened by corrosion the curling or untwisting tendency is unopposed, and the stronger portions on either side of the weakened spot untwist, thus exerting a force contrary in direction on each side, but concentrated in action at the damaged portion.

This is the whole secret—this is the *mysterious agency*, and thus it is that the most serious cause of injury to submarine cables must be attributed to the iron wires.

We think we have shown that iron wires for deep-sea cables are not only undesirable, but deleterious. That this so-called protective material has caused the loss of the 1865 and 1866 cables we have conclusive evidence to show; and it may reasonably be assumed that the same destructive agency which we have described is still at work, so that it becomes an imperative necessity to endeavour to provide a remedy, or at least to try to mitigate in some degree the evils arising from the cause named.

The following opinions will, we think, be taken in evidence in support of our statements respecting the '65 and '66 Atlantic cables:—

At the half-yearly meeting of the Anglo-American Telegraph Company, held February 7th, 1879, Lord Monck said: "The failure of the 1866 cable was caused by oxidation of the iron wires and decay of the hemp."*

Mr. W. H. Preece, in a Cantor lecture, delivered April 28th, 1879, said: "Submarine cables are really practically of only one type. . . . The form of the Atlantic cable of '65 and '66, which is similar to that of many other cables, was deficient, inasmuch as, while it possessed sufficient strength to enable it to be submerged, it had not sufficient durability to retain that strength to enable it to be brought to the surface again in a sufficient length to enable them to effect repairs. . . . Sometimes the cables rest on corrosive stones, copper ores, and ironstone, when corrosion sets in and causes the cables to speedily fail."

The following communication in regard to the abandonment of the effort to recover and repair the 1866 Atlantic cable appeared in the London morning papers of July 28th, 1878:—

"The steamships 'Seine' and 'Calabria,' which were sent out by this Company at the end of May for the recovery and repair of the 1866 cable, returned to London this morning. The cable has been hooked several times, and partially raised to the surface; but the outside iron wires were so weak from the effects of oxidation, that its repair was considered impracticable by the engineers in charge of the expedition.

"(Signed) HENRY WEAVER,
"*General Manager.*

'Anglo-American Telegraph Company, Limited,
"26, Old Broad Street, London, E.C."

Mr. John Pender, M.P., stated, December 4th, 1878, "that it had been proved that the sea, instead of destroying the gutta percha, had the opposite effect: *it was only the iron wires that were affected.*"

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* Hemp is practically indestructible, excepting when in contact with iron.

Sir Willian Thomson has also stated "that the presence of carbonic acid in water is the great cause of iron being corroded."*

We could continue to produce evidence of this description, and we have no doubt that there are some belonging to this Society who are able to endorse the statements already advanced, viz., that gutta percha is practically indestructible, but iron wire has caused the loss of many miles of submarine cables, and that, excepting when in contact with iron, vegetable fibre is indestructible.

We could cite numerous instances of cables having been lifted from considerable depths solely by means of the yarn portion of the fabric, which retained its strength sufficiently to bear the superincumbent weight of oxidised iron wires. The core serving, which is composed of common jute yarn, has time after time done duty as the strength-giving material, although only intended as a padding for the iron armour. This core serving is invariably recovered in a wonderful state of preservation, but the yarns which envelope the iron wires suffer deterioration whenever the latter become oxidised.

The testimony with regard to the enduring qualities of vegetable fibrous material is, in our opinion, so overwhelming, that we are justified in believing it will be readily admitted that a cable constructed without iron wires, and with the strength-giving material composed of hemp, will not possess within itself the elements of decay.

In the case of close-armoured cables covered with an outside serving carrying bituminous compound, oxidation is certainly retarded, but on some bottoms this is only the case to a very slight extent, so that it must be admitted that, wherever corrosion sets in and destroys the iron wires, they have proved themselves a source of weakness instead of strength; for where repairs are effected to cables in which the iron wires are useless, and the yarn portion is the means by which the repair is accomplished.

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* *Vide* table showing amount of carbonic acid contained in sea-water at various stations in the Atlantic. Appendix C, "Voyage of the 'Challenger,'" Vol. II., page 384.

is plain that the iron is only an encumbrance. If the iron armour has to be protected by means of vegetable fibre and bituminous compound, why cannot a less perishable material be more effectually protected with the same coatings?

Whenever close-armoured cables suffer corrosion, the wringing asunder of the core is more likely to occur than it is in the case of a strand cable; for if the corrosive action is confined, as is often the case, to one particular spot, on either side of which there is a considerable length unattacked, the untwisting of the armour at the weak place exerts a more powerful force than that which would follow under similar conditions on a strand cable. If the deterioration were general for any considerable length, there would be less danger of rupture, for then the turns resulting from the untwisting would be distributed.

We are aware that various types of light cables have been devised, and that the question has been discussed from time to time by the members of this Society and by others; but the subject has been allowed to drop, partly on account of the fact that some unsatisfactory feature has rendered the adoption of the suggested type undesirable, and partly because of the very strong feeling which exists in favour of heavy armour; and we must say that, were it not that our convietions compel us to arrive at conclusions in favour of the entire abolition of the use of iron wire for the deep-sea portion of an Atlantic cable, we should hesitate before venturing to do otherwise than accept the opinions advanced in support of the use of such material.

We are convinced that a renewal of this question cannot fail to prove beneficial to the cable companies and the public generally; and we are sure it will meet with that consideration on the part of this Society which so important a subject demands.

We will now proceed to state what, in our opinion, is the most suitable type of cable for the deep Atlantic.

The strength-giving material, by means of which the core can be submerged and recovered, should consist of fibrous yarns or strands made up in such a manner that any twisting or wringing of the fabric is prevented; and, in order to accomplish this, we lay over the insulated conductor a series of spiral or helical servings of non-metallic fibres, yarns, or cords, laid alternately left and right, *the fibres, yarns, or cords of each serving being twisted in a direction the reverse of that in which they are wound about the insulated conductor.*

This point or feature, although at first sight a trifling consideration, is of the utmost importance, as it constitutes the gist and substance of the principle by means of which any twisting or knuckling of the cable is prevented; for it will be seen that, if the yarns or cords of each serving be similar,—that is to say, if they be twisted in the same direction in forming them,—the cable will kink, notwithstanding the servings be wound alternately in right and left hand spirals about the core.

In the accompanying drawings, Figs. 1, 2, 3 and 4 represent the yarns and cords with which the insulated conductor may be served.

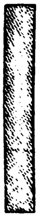
Fig. 1.

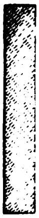
Fig. 2.

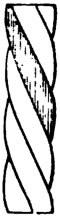
Fig. 3.

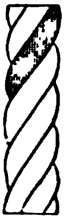
Fig. 4.

and Figs. 5 and 6 represent completed cables made up of such yarns and cords.

In the case of a cable constructed as shown in Fig. 5, it is obvious that the natural tendency of the first layer or serving to untwist will have the effect of twisting up, and consequently hardening, the yarns composing such layer, as the yarns are laid on in the opposite direction to that in which they are twisted.

So, likewise, the following layer or serving, being composed of yarns twisted oppositely to those of the first layer, and wound around in a reverse direction, will not only oppose any untwisting tendency of the preceding layer, but be hardened and consolidated in like manner, and the whole fabric will be compensating in character—that is to say, the outer layer will prevent the inner layer from causing any kinking or knuckling, and the like effect will be produced on the part of the inner layer.

The same result will, it is obvious, be produced by means of the cords shown in Figs. 3 and 4, when laid on in the manner illustrated in Fig. 6; and it is also equally plain that the laying on of the spirals may begin either to the right or to the left, the

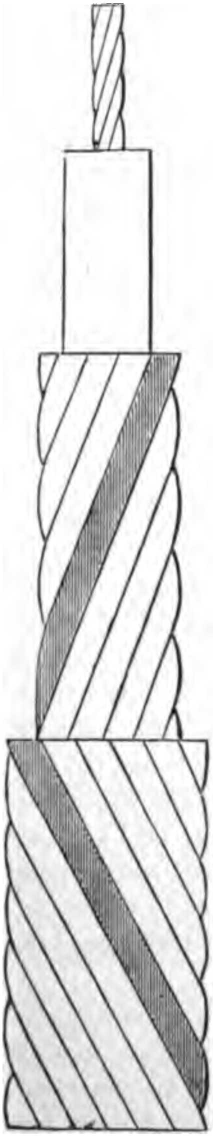

FIG. 5.

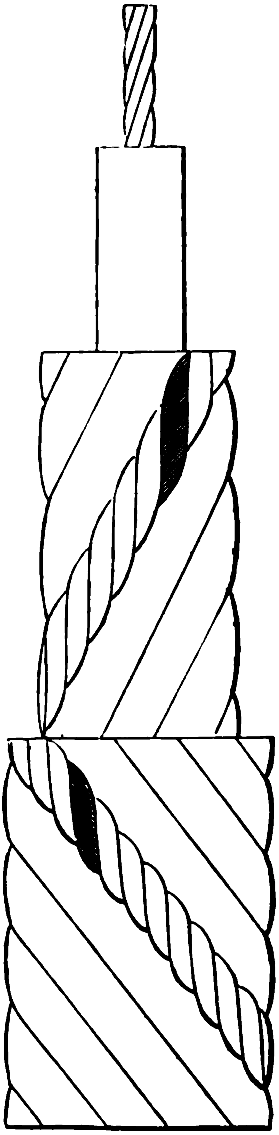

FIG. 6.

essential point in this regard being that succeeding spirals shall reverse their direction.

The servings may be coated, saturated, or filled with tar, bitumen, asphalt, or other preservative material, and may be wrapped with yarn or tape.

A cable such as we have described would not only be more durable than an iron-armoured one, but could be recovered with ease and certainty from any depth.

Strength, lightness, and durability are, we affirm, among the chief requirements in a submarine cable, and these qualifications are to be found in the type which we now submit to the consideration of this Society. We have reduced this paper to the smallest possible limits, but trust that our remarks will give rise to a thorough investigation, and that we shall have the opportunity of studying the views of others before going further into details.

The President: If I may be allowed to use a phraseology which has often been heard in Westminster, I would say that Messrs. Trott and Hamilton have moved for a new trial of a very old case, which has been before the Court on several occasions. I refer to the case of Hemp *versus* Iron, first tried about 31 years ago, as to which should be the protector of, at that time, a young stripling christened "G.P. core." The trial extended over several years, and created a great deal of interest, but eventually the verdict was given in favour of Iron. My whole interest in those days was wrapped up in that stripling; for I knew sufficient of his character to predict that, if used well and properly cared for, he would grow to be a great benefactor to the whole human race; and I am proud to think that I have been spared to see my prediction more than realised.

I watched this case with great interest, because it occurred to me that it was from no special love to "G.P. core" that the contending parties fought so tenaciously for his protection.

When, in 1856, it was proposed to lay a cable across the Atlantic, Hemp moved for a new trial, on the ground that Iron was not a suitable protection for so great a distance. On this occasion over 70 different specimens were submitted to experiment by Messrs. Hemp and Iron. The verdict was again given in favour of Iron; and no doubt most of you are aware that the first Atlantic cable consisted of a G.P. core with a serving of hemp, around which were laid 18 strands of iron, each strand being composed of several small wires. It is unnecessary for me to go into the details of the manufacture and laying of that cable. Suffice it to say, that the first attempt to lay the cable in 1857 failed. In 1858 the attempt was more successful, and the cable was laid; but it had been so knocked about and badly used, that poor "G.P. core" was nearly dead when landed on the shore of Newfoundland. But the fact should not be lost sight of that the laying of that cable did a great deal towards the ultimate success of Atlantic telegraphy. Previous to the laying of that cable, men whose opinions in nautical matters were considered of importance, stated that it was impossible to lay such a cable; and there were electricians who boldly asserted that if it were laid it could not be worked. The actual laying of this cable, bad as it was, put all those gentlemen to silence, for it proved that a cable could be both laid and worked.

But the whole history of the expedition encouraged Hemp to move for a new trial, on the plea of Iron's cruelty and total unsuitableness for the work required to be done. Iron retorted that Hemp had proved itself to be more unsuitable, and instanced how lamentably it had failed in the Irish Channel, where, in the nterstices of a large three-strand hemp rope, two insulated wires were placed. In the attempt to lay the same from Donaghadee o Port Patrick, it was with great difficulty they got it to leave the hip, and when it did go overboard it would not sink; and after sing every available piece of iron on board, from the boatswain's marlin spike to the cook's pannikin, the cable was abandoned to s fate. Iron also instanced the loss of eight hundred miles of emp cable in the three attempts to connect Crete and Alexandria, nd that eventually iron was employed and proved successful. he hearing of the case extended over five years, and at length nished with a kind of compromise, that is to say, it was decided hat the next Atlantic cable to be laid in 1865 should consist of a .P. core served with hemp in the usual manner, and then surrounded with ten solid iron wires, each wire having been previously covered with hemp. It is well known that, after having successfully laid 1,200 miles of that cable, it parted while endeavouring to recover a fault that had passed into the sea, and the expedition returned unsuccessful for that year. But in the following year, after having made sufficient cable for a new line, it was successfully laid without a hitch; and then the lost end of the cable of the previous year was recovered, and that line was completed also, thus establishing communication between England and America by the lines known as the 1865 and 1866 cables.

With these preliminary remarks, I will just refer to one or two of the points mentioned in the paper.

First, an expression is used in the paper which is new to me, that is, "the wringing asunder of the core by the iron wires." I have never heard of such a thing as that. From it one would imagine that if a piece of core were twisted it went in two at once; but let me ask, How many twists will a length of core stand before breaking? I cannot say, because I never had the patience to go far enough, but I have twisted it fifty times without obtaining a break, and then the core was apparently uninjured. The copper strands will show that they have been twisted into shorter lay, but the core cannot be pulled asunder, and I do not think that there is an instance on record (I cannot think of on where a cable core has been twisted in the manner mentioned in the paper.

Next,—"We now have to consider the most serious of accidents to which cables are liable during the process of s mersion." Well, gentlemen, I may mention that the Company to which I have the honour to be connected—and I refer to it because I can vouch from immediate personal experience fo everything I state connected with it—that Company has l 94,000 miles of submarine cable, and yet never once have I kno of a cable parting whilst being laid.

Further, the authors of the paper have quoted from a b written by Dr. Russell in 1865, and what is quoted is not wri of that gentleman's own knowledge, for it will be found in preface of that book that reference is made to the cable attem to be laid in 1857. I must say, that between 1857 and now we ought to be better informed than to have to go back to that book for what has occurred in submarine cable laying. If Messrs. Trott and Hamilton were about to introduce certain improvements in steamers, surely they would not base their idea of improvement upon what they had read relating to the first steamer that crossed the Atlantic, ignoring all that has been done in the meantime; and that appears to me to be similar to what they have done in quoting Dr. Russell's book in relation to submarine cables.

It is also stated in the paper that there is a "mysterious agency existing in the depth of the ocean." I am sorry that Sir Charles Bright is not here this evening, because he had more to do with the 1857 cable than I had; but I do not think there was anything mysterious about it. It was a badly made cable, and was pulled about terribly; and, though it proved the two important facts I have spoken of, it ought not to be mentioned any further in connection with submarine telegraphy. Further, the paper says, "It is greatly to be regretted that records such as Dr. Russell's have not been published concerning other and more recent cable expeditions." In reply to that I would say, happy is the cable that has no history.

A remark was made, which I heard some one slightly applaud, and that was—"We should certainly be wanting in ordinary perception if we failed to read between the lines when studying some of our records concerning Atlantic cables." I do not exactly follow what that means, and shall be glad if the authors will enlighten us upon the records that they have, and let us know exactly what has been their actual experience, and give us some of the details.

Again,—"This brings us to the question, Why do cables suddenly sever in deep water whilst being submerged or when being picked up, and even after being successfully submerged?" I have said that I know of no such case having occurred, though my experience covers 94,000 miles of submerged cable.

Further on it is stated—"Consequently, any slack given is apt to result in a kink or a succession of kinks, which is followed by a wringing asunder of the fabric when the strain is renewed.

This accounts for most of the misfortunes which have happened in submerging some cables, for whenever it became necessary to stop or slacken speed in shifting from one tank to another, or for some other purpose, the risk of this accident has always been incurred." If Messrs. Trott and Hamilton had read further on in the book from which they have quoted, they would have found it stated in connection with the 1865 cable, at the laying of which Dr. Russell was present, that "after we got 80 miles from Valentia, a fault was reported, which was 10 miles to the stern before it was detected, consequently we had to stop." That monster ship the "Great Eastern" had to hang on the cable, and 10 miles were hauled in before the fault could be got at. This was done successfully; therefore, if there had been any of this twisting or kinking that has been spoken of, how was it that it did not exist in that 10 miles of cable? "When we got into water of 2,000 fathoms another fault occurred, and we had to pick up 3 miles;" also, after paying out 1,200 miles, another fault occurred, and the "Great Eastern" had nothing but that small cable to hang to, and after being much pulled about the cable became chafed, snapped, and went overboard; but when the end was recovered, no kinks, twists, or anything of the kind were discovered. Investigation showed that the failure of the 1865 cable was due to the iron wires breaking in many places, and stabbing the core as the cable was being paid out.

As regards the letter from Mr. Weaver, quoted in the paper, to the effect that the 1865 cable was abandoned in 1878, bear in mind, gentlemen, that the laying of the Atlantic cables in 1865 and 1866 were simply experiments: no experience of deep-sea cables had been obtained for the guidance of those who attempted it, and no doubt the machinery employed contained many imperfections. The two cables of 1865 and 1866 were identically the same, except that part of the 1866 cable had no tar on the hemp, and the iron wire used was better selected; but when abandoned, the 1866 cable had been doing work for some 12 years; and all who are acquainted with the history of the early days of submarine telegraphy could not expect that those cables would last much longer.

Vegetable fibre, states the paper, is indestructible. That, of course, would much depend upon the water in which it is placed. I much doubt if a hempen cable would last if laid along the African coast or in the Mediterranean, where, in the case of a cable that was partly laid and then abandoned, the hemp was after a time entirely eaten off.

At one time I thought that iron had proved itself unsuitable for submarine telegraphy, so, turning my attention to hemp, I constructed a cable and submitted it to the Atlantic Company. That cable consisted of a gutta-percha-covered core, the copper weighing 180 lbs. per nautical mile, and the gutta percha 212 lbs. Around that core I put what was known in the trade as "8 lee sealing cord," laying it on the core in, we will say, the right-hand direction. Over that I placed 17 strands of the same cord, but in the reverse direction to the first layer, as specified in this paper. It was a very light cable and weighed only 12 cwt. in air, and it bore 5¾ miles of its own weight in air and 29 miles of its own weight in water, and it lifted 4 tons before breaking. That was, I thought, a very satisfactory cable, and it was put into water and kept; but six months after, on examination, I found it in the most extraordinary shapes, the hemp used having knuckled up the core, and therefore, if that cable had been accepted, the owners would have met with difficulties before it was laid.

With the last paragraph of the paper I agree most heartily, viz., that "strength, lightness, and durability are, we affirm, among the chief requirements in a submarine cable." That is just what we do want; but I cannot agree with the type that has been proposed this evening, and, unless the results of a long series of experiments were known, no Company would be justified in adopting it.

The grapnel which Mr. Kingsford has brought before us and described to-night is undoubtedly very ingenious, but if it had not been tried as stated, I should have thought it impracticable. There is one question that strikes me. Mr. Kingsford says, "We are perfectly awake, however, to the fact that a grappling rope should be made in a manner that will not allow it to kink, and engineers should avail themselves of such a rope, especially in deep water. Patents have lately been granted to Messrs. Trott and Hamilton for a form of rope or cable answering all the requirements of this work." Is the rope here referred to the same as mentioned in Messrs. Trott and Hamilton's paper? and, if so, would these gentlemen kindly inform us how long they have used it, and what experience they have gained with such a rope?

I am afraid I have detained you rather long, for I have gone over, in my remarks, a period of over thirty years of my life, and perhaps the scenes, plans, and incidents that have sprung up before me have caused me to wander a little; but I hope I have made myself sufficiently clear.

Mr. H. C. Forde: The proper form of cable to be used in deep-sea telegraphy is a question that I have for many years taken a great interest in. At one time, I confess, I was very much in favour of the very lightest kind of cable; but when we look into the history of what has taken place with regard to light cables, I think one must hesitate before coming to a decision to adopt the very light form of cables. I would just make a few remarks as to what has been done as regards some of the past cables. In 1855, a cable (a bare core) was laid between Varna and Balaclava by R. S. Newall & Co., the deep-sea portion of which was 300 miles long: this core consisted of 70 pounds of copper solid conductor and 170 pounds of gutta percha to the mile. That cable was successfully laid, in somewhat deep water; it worked for about nine months, and then broke near the Crimean coast. That cable never was, nor probably could never have been, recovered. In the same year a cable was laid between Varna and Constantinople, 150 knots in length, the total weight of which was 15 cwt.; it was sheathed with 12 small wires, and had a solid conductor, No. 16 B.W.G. It did not last long, and was never repaired. In 1857-8, as has already been said, the Atlantic cable was laid, and was partially successful; its weight was only 16·3 cwt. in water; it was sheathed with 18 strands of 7 No. 20 iron wires—in all, 126 wires. Then, in 1857, there was a cable laid between Cagliari-Malta to Corfu (18 No. 14 iron wires), the first section of which lasted one year, and the second section two years. Then, in 1858, an attempt was made to lay a hemp-covered core from Candia (Crete) to Alexandria (I was present at the laying of this cable), and when about 230 knots in length of this cable was paid out, the insulation, from some cause or other unknown to those on board, became very bad, and we found it necessary to stop and buoy the cable in 1,500 fathoms, in very bad weather. Two days after, when attempting to lift it, the cable parted and was abandoned. The cause of failure was not understood, but in all probability the core, from the shrinking of the hemp, got strangled, and its insulation destroyed. After that a similar cable to the Cagliari-Malta and Corfu was laid down the Red Sea to Aden, and on to Kurrachee, its length being about 3,000 miles, and weight in air about 20 cwt. and in water 16 cwt. The Red Sea cable lasted only nine months, and the Aden-Kurrachee portion about six weeks, but this failure I attribute more to the excessive tight paying-out on the part of the contractors, and to a very rough and rugged coral bottom, than to any other cause, this tight paying-out was strongly alluded to in my report to the Directors immediately after the laying of the Aden-Kurrachee section—the laying of which I witnessed. Another form of light cable was tried by my late and valued friend Sir William Siemens; and here [producing it] is a sample of the cable that he laid between Carthagena, in Spain, and Oran, in Algiers. That cable consisted of a core consisting of a copper conductor weighing 72 pounds per knot and insulated with 144 of gutta percha, which was served with two separate and opposite lays of fine hemp strings, put on with tension, then four strips of sheet copper under moderate pressure. The string was put on with a long lay, almost straight, so that every string came into action, and was kept in its place and protected by the outside copper tape. The life of this cable was exceedingly short, as it broke in deep water about ten miles from the Spanish coast, and, so far as I know, it was never recovered.

Other similar cables to this were, I think, afterwards laid between Algiers and Sicily, and also in the Black Sea, with similar ill-fortune. After these failures, it happened that H.M. Government, in May, 1859, determined to lay a line between England and Gibraltar, and my former partner (the late Mr. Lionel Gishome) and I were appointed their engineers to carry out the work. For certain reasons best known to themselves, this work was required to be done in a very great hurry, and we at once reported in favour of a certain size and description of core, and let the contract accordingly. Sir C. Bright and the late Robert Stephenson were also consulted about it, and also sent in recommendations. The question of core being settled, we asked for time to make experiments as to the best form of outer covering to be adopted for the deep-sea portion of the line. At the request of Mr. Gishome and myself, the late Sir William Siemens (then Mr. C. W. Siemens) was associated with us in this work; and we were also ably assisted in our experiments by my late greatly-esteemed friend Mr. C. Laws. These experiments lasted over a space of some six months, and the results have been published in great detail in the joint report of the Board of Trade and the Atlantic Telegraph Company, published in 1860. First of all we tried bare core, consisting of 400 lbs. of copper and 400 lbs. gutta percha per nautical mile—in all, about 7·15 cwt. in air and 3·36 cwt. in water. Its ⅔ breaking strain was 5 cwt., and with this it elongated 26 per cent.: this breaking stress represented 1,500 fathoms of its own weight in water. That core could have been laid most easily in any depth of water, for it only depends upon the speed at which the ship proceeds and the core is shoved out of the ship; but though it could have been laid, it certainly could never have been recovered if any accident had happened to it at the bottom in deep water.

After that a very long series of experiments were made with hemp, and hemp combined with iron and steel wires. Although great comparative strength can be got out of merely hemp-covered cables, they were found, as the President has said, to shrink severely when immersed in water, and consequently strangle and destroy the core, no matter how the hemp was applied. We found that the strongest form of cable was that composed of iron or steel wires enveloped in hemp strands; and we eventually reported in favour of that form, and a specification was accordingly drawn up for this pattern of cable. But that form of cable was not made nor laid on this occasion between

England and Gibraltar, because Lord Derby's Government went out of office, and was succeeded by Mr. Gladstone's administration, who decided not to lay the cable between those two points, principally, I believe, in consequence of the greatness of the depth of water, viz., 2,700 fathoms. The destination of this cable was then altered, and it was arranged to lay it between Rangoon and Singapore, in order to bring China into closer proximity to England; but this idea was not carried out, and it was eventually laid between Malta and Alexandria, as a shallow-water coast line. The type of cable being that which was designed for depth from 200 to 600 fathoms, the core being sheathed with 18 iron wires of No. 11 gauge, this was, in fact, a deep-sea cable laid in shallow water. It lasted some years, but, unfortunately, in consequence of the unevenness of the bottom and shallowness of the water, it was constantly breaking, and was finally abandoned, and was superseded in 1868 by a direct deep-sea cable. The hemp-and-steel cable that we finally adopted consisted of 12 No. 14 steel wires, and that proved a very strong cable indeed: its breaking strain was from 80 cwt. to 147 cwt., which represented from 7,500 to 11,000 fathoms of its weight in water. That was the form of cable eventually adopted for the 1865 and 1866 Atlantic cables.

With reference to the causes of decay and rupture of cables in deep water, I do not suppose that chafing has much to do with it; but what we do find is that, in laying cables, it is perfectly impossible to lay them in such a way that they will lie for their entire length on the bottom of the sea. We find such inequalities, even in the Atlantic and other deep seas, that cables must unavoidably get suspended from one point to another. The cables go out at a very small angle with the horizon, and frequently in 2,000 fathoms there are 20 or 30 miles of cable probably suspended in the water from the stern of the ship to where the cable touches the bottom. Cables are, as a rule, according to my ideas, laid at too great a speed, and consequently the cable, while sinking, often hits a prominence in the bottom, somewhere near the ship, before the further portion of the cable has had time to sink to the bottom; consequently they become suspended, and that has been found to be the case in the 1865-66 Atlantic cables, over which so much money was spent in trying to repair them. No doubt they became broken in several places in consequence of several suspensions due to a certain extent to fast paying-out, and possibly to too little slack in parts.

As to boring insects, I do not think they have caused any faults in the Atlantic, nor do I think any instance can be found where boring animals have actually caused a fault in deep-sea cables, though instances have occurred in 1,000 fathoms of cores being marked on the surface, apparently by boring animals. I must say that, although I was rather in favour of hemp-covered cables at one time, I cannot give that form any support now. I know that iron wires enveloped in hemp strand, when they corrode, destroy the fibre of the hemp, especially that of Manilla hemp, but not so much the case with regard to Russian hemp; and when such cables have to be picked up after several years submersion, we certainly depend on the strength of the iron, and not at all on the strength of the hemp. Cables sometimes get entangled in the bottom, and, when a heavy picking-up strain comes upon them, they frequently break from that cause.

With regard to the durability of cables, the Atlantic cables of 1865 and 1866 lasted 8½ years and 10½ years respectively; but as these types of cables have been improved upon from time to time, their duration of life is now greater. The 1869 Atlantic cable is now 14 years old, and has been twice repaired in very deep water after many years' submersion, the last serious repair having been accomplished in 1882.

The direct Malta and Alexandria deep-sea cable has been 15 years lying at the bottom without any repair being required, and there is every reason to expect it may last for many years longer without any serious expense to the Eastern Telegraph Company.

In 1870, a cable was laid between Suez, Aden, and Bombay (13 years ago), and that cable still continues to be quite good and serviceable, especially in the deep water, and has only occasionally required repair in shallow water, and where the water suddenly deepens.

The Atlantic cable of 1873, now 10 years old, is still in good order, and has never required any repairs in deep water.

The Atlantic cable of 1880 was of an improved form, of which I have a specimen on the table before you. It is, to my mind, the best form of deep-sea cable that I know of, and I expect will be very long lived. Every iron wire is enveloped in a compound of gutta percha and Chatterton's compound, and is further protected with tape prepared with stearine, which will, I have no doubt, prevent corrosion for a very long time.

I should mention here, that when making the experiments years ago, which I have referred to, a somewhat similar form of cable, which I now produce, was then made by the Gutta Percha Company, in accordance with the then views of the late Sir W. Siemens and myself, where each iron wire is covered with gutta percha. This specimen with only iron wire was tested, and broke with a stress of about 55 cwt., which was equal to about 4,700 fathoms of its weight in water, and had that been laid to Gibraltar (and no doubt it could have been laid), I believe it would have proved to be one of the most durable cables ever submerged.

Mr. F. R. **Crampton**: What was the breaking strain of that cable?

Mr. H. C. **Forde**: 55 cwt., or 4,700 fathoms of its own weight. The wires were of iron, and had they been of steel, of course the breaking strain would have been very much greater. We generally like to have about three or four times the strength that is necessary for its suspension in the deepest water—that is to say, if a cable has to be laid in 2,000 fathoms, we should require a cable strong enough to support a length of 6,000 to 8,000 fathoms of its own weight in water. The recent form of deep-sea cables which I and my partners have been connected with fulfil these conditions, and their material strength varies from 5 to 6 tons.

With reference to the twisting and curling of cables mentioned by Messrs. Trott and Hamilton as a fertile source of failure, I cannot say, although I have been engineer for the laying of some 60,000 miles of cable, that in all my experience I have found this phenomenon to occur, nor do I believe any cables have broken from this cause.

As to picking up laid cables in deep water, I remember, when the Red Sea to India cable had to be picked up in fairly deep water, on account of a fault, 20 miles were raised at the rate of about 2 miles an hour without a hitch of any sort, and certainly no break occurred from twisting or curling, and this was an ordinary iron-sheathed cable. I have also been out on the French Atlantic repairs, and have seen several miles of cable, some 12 years old, picked up in 1,700 fathoms of water with the greatest possible ease.

On the last occasion on which my late partner, Mr. Herbert Taylor, was engaged in making repairs, a length of 15 miles of cable were picked up that had been lately laid over a very rugged bottom, and picked up to the end where it had been broken by grappling, and there was no sign of damage, or difficulty from twisting or curling on this occasion.

Iron wires, in the course of time, must inevitably deteriorate, but the deterioration is very capricious, and depends to a great extent on the nature of the bottom. In some places they decay rapidly, and in others remain almost perfectly good for ten or twelve years, according to my experience. No doubt, in the 1865-66 repairs, the cable was very weak, and could not be found strong enough in sufficiently long lengths to enable the splices to be made and the repairs to be effected.

I must say that, in my opinion, especially where animals are at the bottom of the sea, it is a very dangerous thing to cover a core with nothing but hemp, for that forms a kind of nest for these boring animals to live in, which gives them a base of operations for making their depredations on the core. If we were to adopt very light cables indeed, I should say use a bare gutta-percha-covered wire: it could certainly be laid, and might last very long; but you must be prepared to allow it to take its chance, for when once broken in deep water it could not be found with a grapnel or raised to the surface. At one time Professor Fleeming Jenkin and I had an idea that it would be a good and cheap form of cable if six small cores were laid across the Atlantic, each capable of working at the rate of, say, four words a minute. In that case, if one core broke, five others would remain to do the work. I made an approximate estimate of such a series of cables, and found that they could be laid for about £100,000. The risk of laying them would be considerable, but their repair in case of accident impossible.

In submarine cables it is a first necessity that a cable should have sufficient weight to sink it to the bottom at a fairly rapid rate; and I doubt very much whether any cable made of hemp would fulfil this condition, or could be made of sufficient strength to be felt at the bottom by means of a grapnel and long rope; and I cannot but believe that hemp will decay on various bottoms, especially where iron is present, just as fast as iron wires will.

In conclusion, I should like to say that I consider Captain Trott and Mr. Hamilton deserve the best thanks of this Society for bringing before them such a carefully-considered paper on such an interesting subject, and upon which no doubt there are still a great variety of opinions.

Considering their great experience in the repairs of deep and shallow sea cables, their views with regard to light deep-sea cables should command the greatest respect; and I for one would be very glad to see the form of cable they recommend put to a practical test between two points, say, 100 miles apart, where the water is as deep as 1,500 to 2,000 fathoms. Without this test being applied, I consider it would be presumption on our part to condemn the use of their proposed cable.

I do not think anything is said in the paper with regard to the cost of the cable the authors advocate, nor as regards its probable duration or material strength.

The grapnel described by Mr. Kingsford is very ingenious, and I should much like to see it tried. We have not heard whether it has been tried in great depths, but if not, I should think the grapnel-rope required would twist and kink unless swivels were used; but perhaps the author will kindly give us some information on this point.

Mr. E. B. Bright: With reference to the paper on grapnels, there appears to me to be a considerable difficulty as regards the insulation of such an appliance to grapnels, which, as all know who have had much experience in picking up cables as well as laying them, are subjected to very rough work on the bottom of the sea, and great hydraulic pressure in deep water. I do not see any provision for attaching a length of chain to the grapnel; but whenever grappling, especially on rough ground,—or a chance of rough ground, and we never know when rough ground may be met with,—it is necessary to attach a considerable length of iron chain to prevent the abrasion of the grappling-rope. I must also say that I agree with our President and with Mr. Forde as to the necessity of using swivels so as to enable the grappling-rope to turn, and shackles to admit of putting on additional lengths of grappling-rope when necessitated by varying depths. I am afraid it would be a very difficult thing to get such an apparatus as described in the paper to work satisfactorily with the rough-and-ready work that, especially over rough ground, grappling means. I think I may say that in grappling for one cable over rough ground, in deep water, I broke or bent fifty grapnels.

Now grapnels of course represent a certain amount of expenditure, and I should very much like to know at what cost such a grapnel as that now before us could be made. If the author has had one made, perhaps he can give us the price.

Captain Trott: We have not had any specially made; so far we have had only those we fitted ourselves on board.

Mr. E. B. Bright: Can you form an idea?

Captain Trott: No, not the remotest. I do not think it would be very much expense.

The President: Perhaps the authors will kindly take notes of the points raised in discussion, for reply, and we shall get on better.

Mr. E. B. Bright: It was a passing matter, and one which I naturally thought had been already ascertained.

It appears to me that in many parts of the world where grappling goes on, signals might be produced in such an apparatus (I do not wish to discourage its introduction, because I like every scientific improvement) by large branches of coral, large stones, sea-weed, and other things brought up, which would be apt to give false signals. I remember bringing up a rock, weighing several hundredweights, that was absolutely jammed in the interstices of the grapnel; and I have frequently grappled and brought up large pieces of coral.

I will now pass to the other paper.

As regards accidents to cables, I think that, as far as the three principal points raised go, abrasion by ice, abrasion from rocky and stony bottom, and injury from anchors, we must all admit that in such cases the stronger the armour coating of the cable the better. There we distinctly require iron or steel or some other means of protection, of not a soft or too yielding character.

As regards other accidents to which cables are liable, I observe that the question of injury from lightning is omitted, but perhaps that need not be commented upon; and also the faults arising from defects in manufacture. As regards the wringing asunder of the core by the iron wires, I have had more or less experience in connection with submarine cables for thirty years, and I have never met with such a case.

On the general remarks in the paper, I would say that the authors have proposed a form of cable which, I think, has been frequently proposed before, *i.e.*, hemp laid in different ways; and we must remember that even the iron covering-wires are not laid with a twist, as seems to be assumed in the paper, but, as is perfectly familiar to us, the twist is carefully avoided. Every machine used for laying on the covering-wire of a submarine cable is so arranged as to take any twists out when putting the wires on.

The principal defect in the present system of cables is ascribed by the authors to a "mysterious agency existing in the depths of the ocean," which is quoted as from Dr. Russell's account of the laying of the first Atlantic cable of 1858, but is really taken from his account of the laying of the 1865 cable; but he was not present at the laying of the first cable seven years before, for he was in India writing his account of the suppression of the mutiny at that time. Therefore his remarks on this point must have been derived from hearsay, and not from the official reports. I venture to say that there are no "mysterious agencies" in laying cables. In 1858, the first Atlantic cable had been stored at Devonport for a year in a shed, and at some points had been rusted and weakened; and the dynamometer of the machine used, which was the first dynamometer employed for such purposes, was defective in its records. I have it from my brother, who laid the cable, that they could not rely upon these records, and that they found them at variance with the facts; and that was the "mysterious agency," coupled with the weakness of the cable from lying in the shed at Devonport between 1857 and 1858.

Except in places where there are very strong currents or under-tow, light cables might be easy to lay; but the question is one of feasibility of repairing, and there are other causes, besides those enumerated by the authors, which occasion faults, such as the development of factory faults in the insulation after submersion, and damage by lightning.

In grappling, it is hardly possible, as we know, to avoid dragging the cable along the bottom for some distance, and even with such an appliance as has been brought before us it would be impossible to immediately pull up a ship, or to prevent a certain pressure and a certain dragging, in which case the hemp-covered cable would have a very much worse chance than one of the usual form. If a cable is not strong enough to affect a dynamometer, I should doubt it being strong enough to be got up in deep water. Examples have already been given of two attempts with hemp-covered cables. I was acquainted with the one referred to by our President across the Irish Channel in 1852, when a cable was attempted to be laid, hemp-covered, between Donaghadee and Port Patrick. That failed, and was found to be dreadfully kinked afterwards. I saw specimens of it shortly afterwards, which I inspected with interest, as it was laid by a company opposing the Magnetic Company, which I then managed. The protection of the wires of submarine cables is of course of the greatest importance; and so long ago as October, 1852, I took out a patent, No. 14,331, with Sir Charles Bright, which included a system of protecting cable wires with a thin metallic riband, combined with a tape, which formed a safeguard against the teredo. But the most extensively employed and successful application against both rust and the teredo is known as Bright and Clark's compound, which has been applied to many thousand miles of submarine cables.

A patent was taken out in 1858 (No. 1,965) by Messrs. Clark, Braithwaite, and Preece, for applying asphalte and hemp to the outside wires of a finished iron-covered cable. It was adopted for a small length laid to the Isle of Man in 1859. The cable was passed through the compound, a revolving tank with bobbins of hemp, and the mixture was warmed by charcoal fires outside. The insulation was damaged by this process, and it required more than a fortnight to coat the 36 miles.

The next step was in February, 1862 (No. 446), when Sir Charles Bright patented the system, generally adopted in all telegraph works afterwards, of applying a preservative compound consisting of certain proportions of mineral pitch, tar, and powdered silica, the latter being devised by him for protection against the boring powers of the teredo. The tool of the teredo when it comes against silica is broken, and cannot penetrate any further, so that if silica is mixed with the protecting coating, the teredo cannot get through and cannot injure the hemp or do any harm to the cable. The heated compound is poured over the cable by an elevator, and the supply ceases on any stoppage of the cable-closing machine, thus preventing the possibility of damage to the insulation by the process. The cable is then passed through rollers with a stream of water flowing over them, by which the coating is thoroughly pressed into all the interstices of the wires. Thus the coating is done at the same time by part of the cable-covering machinery, and the cost, delay, and damage of recoiling avoided. This was immediately adopted for the Pembroke and Wexford cable of 70 miles in 1862; and in the latter part of the same year for the Persian Gulf cable of 1,450 miles, made for the Indian Government to complete the first line to India. The subsequent application has been so large during the past 20 years, and so well known, that I need not recapitulate the many cables. The Persian Gulf cable was ecently stated to be in good order, though laid about 20 years go, and that in a hot climate favourable for the existence of he teredo, and subject to other objectionable conditions.

As regards the failure of the Atlantic cables, the real cause of failure appears to me to have resulted from no preservative system such as the above having been adopted, and from the outer wires being of the gridiron form, and not a "closed" cable, so that on any decay of the hemp the strength was lost.

Mr. T. Russell Crampton: Mr. President and gentlemen,—I have been a little disappointed with the remarks that have been made, in not having heard what is considered as the minimum strength which these light cables should have for laying and for lifting. It has always appeared to me that the minimum breaking strain was the first condition to be dealt with in these cables. When these matters come before me they take me back to 1851, when the first submarine cable was called for. No data at that period existed to guide us. It was, I confess, a serious matter to me at the time. After serious thought, rightly or wrongly I came to the conclusion, that at any rate a light and weak cable for the purpose was unsuitable. A single wire covered with gutta percha, about half an inch diameter, was laid; it was sunk with weights, and messages were passed through it, but on the second day it broke. Still it gave proof that it was practicable to lay such a cable. The question then arose as to what was practically to be done, and it came to my lot to decide. I established in my own mind at that period, that if we were to have a cable at all across the Dover Channel, it must possess sufficient strength to meet all contingencies of laying and lifting for repairs, and to withstand the dragging of the anchors of heavy fishing boats, and even larger ships; and the end of it was a cable was made 1¼ inches diameter, containing four No. 16 copper wires, with ten No. 1 iron wires around it. Some kinks occurred, and I have in my possession now a kink which was cut out three years after it was laid, for curiosity, not because it had caused defective insulation. That cable is at work at the present moment, after thirty-three years service, and is the type of nearly all existing cables. The success of this Calais cable induced gentlemen to come to me for advice and assistance respecting the Atlantic cable project. There was a strong feeling on the part of the promoters for light cables, which I objected to, and it would be interesting to you, perhaps, to know that my declining to co-operate in that great enterprise, was simply because I would have nothing to do with the light cables proposed to be laid across the Atlantic.

The proceedings of the Institution of Civil Engineers, 1856-57, will show that the project for the first cable across the Atlantic was discussed in this room, when he stated, at the time the first Atlantic cable was under construction, as follows:—"Mr. Crampton remarked that several questions which had been asked in the course of the discussion had not yet been answered. First, as to the strength of the cable. He believed that the breaking strain would be about 3 tons, and, in his opinion, that was not sufficient to guard against the contingencies that might be expected to arise. . . .

."Having given the subject a great deal of consideration, he had arrived at the conclusion, that the proposed Atlantic cable should be made of such material as to afford the greatest strength with the least weight, taking care that the weight was sufficient to sink it rapidly. The iron-wire covering adopted did not fulfil that condition. Now a cable could be made of steel, of the weight of about 1 ton to the mile, of sufficient strength to meet almost any contingencies that might arise. Sudden changes in the weather were most to be feared in the laying of the cable. Recently, when repairing the Calais cable, one day they had fine weather, and the next day a gale came on which obliged them to run into Ramsgate.

"He had hoped that more definite opinions would have been expressed on the important question of the Atlantic telegraph, and the silence which had been observed might lead to the belief of the proposed plan being generally approved by the profession. This, however, was not the fact; contingencies should be prepared for, which was not the case with the present cable, and he must record his opinion that the risk incurred was at least imprudent."

My view was that from 7 to 8 tons breaking strain should be provided for in dealing with this kind of cable. The cable which failed had, I believe, a breaking strain of something like 3 tons. I may have been wrong at the time,—I do not say I was not,—but I feel convinced that most gentlemen present who are dealing with this question are adopting very nearly the same breaking strain, *i.e.*, of 7 or 8 tons to the inch, for any reasonably light cable that they now make.

With regard to the grapnel, I cannot see any inconvenience in it. The cost, if it answers the purpose, is not worth consideration. I think the proposed grapnel should be encouraged, particularly for light cables, because a light cable has little effect upon the grapnel under ordinary circumstances; but if a connection can be produced with the pressure caused by the light cable, it will be a great advantage, and one that should be well considered by those gentlemen who are more engaged in these matters than I am.

Mr. W. H. **Preece**: I should much have wished that this discussion could have been adjourned until another evening; but the fact is, that the gentlemen who have come from the other side of the Atlantic, and have given us the benefit of their practical experience, will not be here the next time we meet, and we must have from them some little further explanation.

I do not think it has been sufficiently pointed out that these three gentlemen speak from actual experience in repairing these very cables that have been brought before us, in deep water, and under similar circumstances to those which have been alluded to by Mr. Forde and by Mr. E. B. Bright. I should like to have asked the authors, in the early part of the discussion, to explain a little more fully what is not very clear in the paper in regard to the "wringing" action. They have brought before us a distinctly new fact; and it is quite impossible for gentlemen of their practical experience to have dared to bring before a Society like this a fact such as that to which they refer, if they had not very good evidence indeed of the existence of some action in deep water upon cables which can be explained by this so-called wringing effect. Therefore the first thing that I should ask the authors of the paper would be to explain a little more fully what they mean by this wringing force. But more than that, it appears to me that my friends at this table who have brought their guns to bear upon the authors of these papers have lost the main point of the papers —that is, not that there have been hemp cables before, and not that there have been two layers of hemp laid in reverse directions but that they have brought before us a cable so designed and so constructed as to resist this peculiar wringing force that is said under certain circumstances to produce rupture. Now, cables have gone in deep water; cables are supposed, as Mr. Forde has said, to lie suspended from point to point, and then to break from weakness due to decay. This is very often given as an explanation of the loss of cables; but I take it that the main point in the paper read by Captain Trott and Mr. Hamilton is this, that you need not assume these indentations in the bottom of the ocean—you need nót have suspension from peak to peak; there is another cause. If they give proof to our satisfaction that there is a wringing force in a cable which comes into action when the motion of the water or other cause decays the outside wires so far as to reduce their strength, I can easily understand that if a cable has a twisting force—I do not say that it has, I cannot speak from experience, but Captain Trott and Mr. Hamilton, with their experience, say that it has—it will break. If that be so, they have certainly brought something new to all of us. I have paid very great attention to the life, the durability, and the history of submarine cables, but this effect is quite new to me, and I am sure it must be new to a great many present; and I would beg of the authors to let their reply be particularly directed to the further explanation of this point, so as to prove to our satisfaction the existence of an unexpected force, or a real "mysterious agency" at the bottom of the Atlantic.

The **President**: As time is getting on, and it would be very inconvenient to adjourn this discussion until our next meeting, more especially as the authors of the papers cannot then be present, I will only say that Messrs. Trott and Hamilton asked us for our candid opinion, and I think, as far as I am concerned, they have got it. I will now ask them to reply, and kindly give us a little more information as regards the questions that have been put.

Capt. S. **Trott**: I think I need not go into all the details that have been enumerated, because it matters little to me how many light cables have been made. The first one that is quoted I may as well deal with at once. It has been alluded to by two or three gentlemen, the President, Mr. Forde, and Mr. E. B. Bright. Mr. Forde and I have heard, on a previous occasion, a great deal of talk about cables. This Donaghadee cable, if you will call it a cable,—I do not call it a cable myself,—was simply a four-strand rope with a core in it, made up in short lengths of 140 fathoms each, and afterwards spliced together, probably made without any tar, or it may have been white manilla. I saw the maker of it, Mr. Baker, a few days ago, and he gave me its history. It is not the kind of rope I would for a moment recommend for a telegraph cable. Here is a piece of grappling-rope, and a question was asked what we intended to do with the grappling-rope attached to the indicating grapnel. This rope is made to our specification, and cannot twist. It is not the best specimen that I would like; it was made in a hurry to bring here. In this rope is a core, and it will stand what it is intended for, although we would not like a cable of that description. Doubtless a great many light cables have been made, but I see from the remarks that have been made that they nearly all contained more or less of iron-wire covering, and they nearly all have parted in some mysterious way by some "mysterious agency" (I will not give up that term, it was not of my coining, but Dr. Russell's); and there is a mysterious agency: when the wires decay, certain things take place. I now introduce to you a photograph of a fracture that occurred in a cable while lying on a perfectly level bottom. [The photograph was passed round the meeting.] That cable was not suspended in any way, still the core was severed, and by twisting, as the iron wires show. I do not believe in cables being suspended in the depths of the Atlantic, as has been stated here. An allusion was made to the 1865 cable probably being suspended, but Mr. Hockin, who was on the expedition sent out to repair it, would not state the cause of rupture, when asked in a room across the street here a few years ago, although it was tried to be proved that that cable was suspended. The soundings taken on the expedition alluded to were some by Sir William Thomson's wire, some by grappling-rope, and some by a buoy-rope and mushroom anchor; hence the difference in depth of water obtained. I have taken as many soundings in the Atlantic as most men, and I know that there must be a great difference between soundings taken with a grappling-rope and soundings with a pianoforte wire: the one is reliable, the other is not. I do not think that all the soundings which the "Challenger" took were reliable, simply because rope was used to a great extent; they might have been much better if taken by wire, as recommended by Sir William Thomson. Now, I would state that my experience proved beyond a doubt that cables do twist, and sever also, as shown in that photograph. This is not the best specimen I have seen; but there is a gentleman present, I think, who was with me on one occasion, representing a company, and he saw an end of cable hove on board with the iron wires reduced to needle points by oxidisation, and the gutta percha and copper wires wrung off in short twists. You will please note in the photograph, that although the wires are themselves reduced to needle points, yet a few inches from the ends they would gauge up to their full size; therefore they have the full helical force left, and the force being concentrated at the weak place, a fracture of the core is the result. It is useless to say that a rope or a cable that is made hundreds of miles in length has no twist in it: it most assuredly has. You may take a rope or any other cord you like; unless it is specially made in reverse layers, as we have stated, there must be turns in it. Those of you who have been cable laying have seen the marks on the cable from pressure in the tanks, twisting round and round after passing over the ship's stern into the sea. I would ask, what becomes of that twisting when the cable is lying on the bottom, and when there is a weak point put into it, as shown in the photograph? I would also like to ask, when a ship stops and has to pick up, and turns run out from the ship's bow downwards, which they assuredly do—I have seen the cable come up quite flat over the sheaves, the lay much elongated, and the core spewing out—I would like to ask you, what becomes of the turns then? Also, what becomes of the turns accumulating on the bottom? I have seen a cable broken, not once or twice, but perhaps thirty times, in picking up to remove faults. And from what cause? Not because the cable was weak—not because it could not bear a sufficient strain! It would break, not from tension, but generally when the ship was still and the cable hanging perpendicular. I suppose some of you have also experienced the same thing when carrying out repairs. I think I have lifted cable in deep water in the Atlantic almost as much as most men, and I think I have done more repairs on cables in shallow water than any other man, and have gone out of my way to see some of the old cables which have long since been abandoned. Speaking of the manner in which cables oxidise, our worthy President will know something of the 1867 cable, which was covered with black iron wires. I picked up some of that cable in Placentia Bay last summer, to see the state of it, and those wires, on good bottom, were certainly quite as good as when they were manufactured. But as soon as we got out of the centre of the bay, where the ground became rough, there was nothing left but the core and its serving, with lumps of stone and rust, but no appearance of wire about it. That is what often occurs, so that the weakest point in a cable is something like the weakest link in a chain, I imagine.

I do not deny that there are some rough places in the Atlantic, and there are some differences in the depths of water. I discovered a very shallow place in the Atlantic, though it is not called by my name, but is known as the Faraday Shoal. Proper soundings should be taken before a cable is laid. I do not think any one would go far wrong then, if from eight to ten per cent. is allowed for slack, which is the plan generally followed, and which for the little inequalities of the Atlantic is more than is required. I am sorry time will not permit me to go more deeply into details.

As regards hemp, here [producing it] is a sample of hemp which I should be glad for you to inspect. It has been under water, and I should like any one present to judge, if they can, for how long it has been under water. Here is a section of the same piece, taken from the clinch of an anchor at a place where it was infested with insects. That is Russian hemp.

Mr. W. H. **Preece**: There is one other question I should like your answering at the same time. When cables break while you are picking them up, will you kindly indicate the position? Where do they break—is it near the ship, or is it at the bottom?

Captain **Trott**: Never near the ship, unless by machinery or something of that sort. In all my experience I never knew a cable to break on deck but once. The Direct cable broke on deck once, but from what cause no one ever knew. The breakages I refer to always occur on the bottom, where there is little or no strain on the cable. Given the depth of water and the type of cable, say, 2,000 fathoms or upwards, I can almost venture to tell you how much it is possible to pick up. It is when ships have passed a fault, and have to go back to pick up, failure almost invariably occurs. Trace back any of the breaks, and I will engage to say that they have not occurred when the ship has been going steadily along and paying out, except something may have occurred in the tanks.

That hemp has been seventy-nine years under water, and is now in as good order as when new.

Mr. H. C. **Forde**: Was it some belonging to the "Royal George"?

Captain **Trott**: No; but some belonging to the "Royal George" was at the Fisheries Exhibition a short time ago, and I am sorry I did not get a piece to bring here. That was between fifty and sixty years under water. I have some yarns of it now, and can tell you they are pretty strong.

Mr. F. A. **Hamilton**: I think it is hardly necessary for me to detain the meeting except for a very few remarks.

Mr. Willoughby Smith stated that hemp was on a new trial; I think it is time that it had one, for iron wire has had a very costly trial.

I will pass over the remarks that referred to cables in shallow water, because you all know that in the paper we speak particularly of deep-sea cables.

Captain Trott has dealt with the light cables which were partially of iron and hemp. Now it has been admitted by one of the advocates of iron that a compromise was necessary between iron and hemp.

As to personal experience with regard to twisting and kinking, I think it quite possible that a great many of those who have dealt with cables may not have had that particular experience; they have been always so fortunate as to lay such perfect cores that only few instances have occurred rendering it necessary for them to pick up in deep water. Immunity from faults in the core has made the work of paying out a very pleasant proceeding; I know that from experience.

I think it was perfectly fair for us to quote from Dr. Russell's book; and I think I might go further and say, that although the old cable of 1858 was alluded to, the same remarks would apply to the cable of 1865, and that is in the same book.

The **President**: No.

Mr. F. A. **Hamilton**: I have some extracts from it here, Sir, and it distinctly states that what had been intended as the cable's protection was the source of its weakness and ruin, and that is mentioned with regard to the 1865 cable.

Mr. Willoughby Smith mentions a mishap having occurred 80 miles from Valentia; that is not in very deep water, I think.

Captain **Trott**: No.

Mr. F. A. **Hamilton**: Of course hemp requires to be protected, as the iron does with hemp. I think, if the iron wire has to be protected by hemp, it is an acknowledgment of its weakness.

As regards light cables, an instance was given by Mr. Forde of a failure 10 miles from shore, but that is not part of the question. Mr. Forde's experience of light cables has been very unfortunate, and is that of many years ago. I rather think that most men would have somewhat modified ideas once held, from observation based on more recent experience.

The **President**: Hear, hear.

Mr. F. A. **Hamilton**: The specimen of hemp cable brought before you to-night is only a hand-made specimen, but we have had this type of cable working under water for eighteen months or more, in a place infested with marine animals, and no trouble or difficulty has occurred.

Mr. H. C. **Forde**: What length?

Mr. F. A. **Hamilton**: It is a short cable laid for a telephonic line. We manufactured it ourselves, and laid it across the harbour at Halifax.

Mr. H. C. **Forde**: It is not a very long one.

Mr. F. A. Hamilton: It is a good test, because injury is more likely to occur to it there than in the deep Atlantic. Concerning the 1880 cable mentioned by Mr. Forde as of an improved form, the iron wires are protected from corrosion by a process which must be very expensive. I would reiterate the remark, lest our statements should be misunderstood, that we advocate our type for deep-sea cables, and not for shore ends, where abrasion is likely to take place.

The breaking strain depends upon the weight of the cable itself. It is a question of the specific gravity: I think that will be seen.

Mr. T. R. Crampton: I meant the actual breaking strain in air.

Mr. W. H. Preece: We do not break cables in air.

Mr. F. A. Hamilton: From 4 to 5 tons in air, in the specimen that we have made; but you can make it any strength you like, taking into consideration the weight. I may say this, that an iron-armoured cable, when new, is very well calculated to inspire great confidence in its strength, but where there is no weight to be borne, surely it is plain enough that there can be no great strain.

I think Captain Trott replied to the question with regard to the twisting or curling tendency, but it is very evident that in picking up a cable the turns are thrown out, and they accumulate at the bottom, and there must be a wringing asunder of the fabric: it occurs invariably when cables are picked up in deep water.

The President: Gentlemen,—It is now very late, but I am sure none of you will regret having been detained, as we have had two very interesting papers followed by an interesting discussion. However much we may have differed in opinion as regards hemp *versus* iron, I am sure there will be only one opinion in what I have to propose, and that is that we give the authors of the papers—Captain Trott, Mr. Hamilton, and Mr. Kingsford—a hearty vote of thanks for ventilating this interesting subject. If it were possible, I am sure your thanks would be more heartily given when I tell you that those gentlemen have presented to the

Society the grapnel which is now on the table, and which may turn out to be a most useful instrument.

A vote of thanks was most heartily accorded.

A ballot then took place, at which the following were elected:—

As Foreign Members:

M. Demétre Colintineano. | Dr. M. J. Grier. | J. A. Sherman.

As Associates:

W. Charles Benedict.
Vivian Bolton Douglas Cooper.
J. C. H. Darby.
A. C. Howard.
A. McCurdy.
R. McGowan.
Sergeant J. McIntyre, R.E.
Alfred Slatter.
W. B. Stainton.
Lord Sudeley.
Alfred Thompson.

The meeting then adjourned until Thursday, 13th December, 1883.

LETTER FROM H. KINGSFORD.

To the Secretary,
Society of Telegraph-Engineers and Electricians.

DEAR SIR,

The meeting of the 29th ultimo having been brought to a close at a late hour, and no opportunity having offered for us to answer the questions put, referring to the grapnel paper read on that occasion,—most present then feeling, I think, that to do justice to the cable paper required much more time than it was possible to accord at one meeting,—I beg to notice herewith the various remarks passed on the first-read paper.

To commence, then, with the President's comment, "The grapnel is undoubtedly very ingenious, but if it had not been tried as stated, I should have thought it impracticable," Captain Trott and I feel that in having accomplished something which Mr. Smith, with his world-renowned ability, would have thought impracticable, we have not lived in vain; and we beg to thank him for the very high (we feel, vastly too high) compliment he has paid us. In answer to the President's question respecting the rope to be preferably used with the grapnel, it is that form mentioned in the cable paper by Messrs. Trott and Hamilton.

It is very gratifiying to Captain Trott and myself, also, that Mr. Forde should consider our grapnel ingenious; and with regard to the desire that gentleman expressed to be on board when it is tried, we can only say how delighted we should be to have the honour of Mr. Forde's presence. The grapnel has not yet been used in two thousand fathoms, but we have every confidence in its efficiency in that or any other depth.

Mr. Forde's idea that the grapnel-rope required in a great depth would kink and twist unless swivels were used, is entertained as well by Mr. Bright, and will be answered in due course.

Mr. Bright says, first of all, that there appears to him "to be considerable difficulty as regards the insulation of such an appliance to grapnels," owing to the "very rough work at the bottom of the sea and great hydraulic pressure in deep water" to which cables are subjected. As will be seen on referring to the diagrams accompanying the description of grapnel, "the insulation of the appliances," to use Mr. Bright's phraseology, is rendered quite safe, such appliances being secured in the grapnel in such a manner that, unless the latter should be ruptured or one of the plungers depressed, neither the core therein nor the contact-plate thereto attached could be in any way touched, however rocky may be the bottom. Mr. Bright's mention of hydraulic pressure seems to me to be singularly devoid of any significance whatever in this case, considering my remark that "the water is allowed free access around and about each separate part, in order that its pressure shall be equal on all sides."

Mr. Bright does not "see any provision for attaching a length of chain to the grapnel," and adds, "But whenever grappling, especially on rough ground, it is necessary to attach a considerable length of iron chain to prevent the abrasion of the grappling-rope." Judging by the grappling, of which I have seen so much during the last few years, I should say that when a grapnel is nursed, so to speak, as it always is by a skilful grappler, such iron chain is not needed on any bottom, however rugged it may be;

but, nevertheless, there is nothing to prevent the grapnel-rope being encased in iron chain or any other easily applied casing for any desired length from the grapnel.

Mr. Bright's next objection is "the necessity of using swivels, so as to enable the grappling-rope to turn, and shackles to admit of putting on additional lengths of grappling-rope when necessitated by various depths."

I regret that my own answer to Mr. Forde's request for information on the score of swivels, or their absence, must serve in lieu of a much more lucid one, which would be tendered were Captain Trott not at present absent from the city. I stated, in my account of the invention, that "a grapnel will turn, and does turn, with the rope; a swivel is therefore of no value." Nor was this a mere dogmatic assertion, stern facts having proved the truth of it over and over again. The turns to which Mr. Bright refers, and which, in the opinion both of Mr. Forde and the former gentleman, render a swivel necessary, are either put in when paying out slack rope,—it being evident that the rope will right itself when the grapnel is in suspension,—or they are put in by the grapnel itself. The result of an equal number of turns, whether put in at the ship or on the bottom, would of course be the same, but in the former case the number of turns would be limited; we will consider, therefore, the alternative. Certainly, when the grapnel turns over on the bottom, the rope will turn also—but what of that? Given any kind of properly-constructed hempen rope, but more especially the kind suggested, with no tendency whatever to turn or kink, and I ask, is such a coincidence possible that the grapnel should constantly meet with obstacles which would all, or nearly all, tend to turn it over in the same direction; that so many turns should be in this manner accumulated in the rope as to injure the contained core; and that all this should take place in the face of the fact that every turn thus accumulated in the rope would be so much energy stored up, striving, of course, to expend itself in bringing the rope again to its normal condition—without a twist: an energy which, when represented by very many turns in a ridiculously short, thick rope, might perhaps be sufficient, in expending itself, to turn a grapnel over with it, but which might be totally disregarded in 100 fathoms, to say nothing of a length twenty times as great? I think now that by the *reductio ad absurdum* process I have shown that no danger is to be anticipated on the score of the rope turning.

As regards the shackles, the grapnel-rope should be long enough for any emergency; and does not the greater include the less? It is not necessary to use all the grapnel-rope you possess. Must we strive to emulate Newton, with his large hole for the cat and small one for the kitten?

Mr. Bright next says, "I am afraid it would be a very difficult thing to get such an apparatus, as described in the paper, to work satisfactorily with the rough-and-ready work that, especially over rough ground, grappling means;" and adds, "I think I may say that in grappling for one cable over rough ground, in deep water, I broke or bent fifty grapnels."

Certainly the work just instanced by Mr. Bright was "rough-and-ready" with a vengeance, and I am happy to say that it has never fallen to my lot to witness such grappling as Mr. Bright mentions. But while I take the opportunity at once thoroughly to disclaim any intention of giving hints on grappling, I would remind Mr. Bright that had he used for his rough-and-ready work such an appliance as I advocate, he might have saved a considerable amount of time and labour, and perhaps some few grapnels, as he would have been advised of ruptures as soon as they occurred, and would no longer have towed a partially or totally disabled grapnel.

As Captain Trott stated, the cost would not be prohibitive; and as a saver of time, labour, and iron, it might even have proved an economy in the somewhat extreme case mentioned by Mr. Bright. Again, it appears to Mr. Bright that substances other than the cable may in some parts of the world act upon the plungers, and thereby give false signals; and he instances a case (probably part of the rough-and-ready work aforementioned) where he brought up "a rock weighing several hundredweights, that was absolutely jammed in the interstices of the grapnel."

That we anticipated the possibility of false signals arising in the manner described by Mr. Bright, is very true; and in order to reduce the likelihood to a minimum, we suggested that guards could be fitted to the stem, "to protect the plungers from the pressure of ooze, etc.;" but I mentioned that in practice we had not found those guards to be necessary, although I may state here, the grapnel has been used over rocky bottoms, and also where sea-weed abounds in enormous quantity.

With regard to Mr. Crampton's remarks, we thank that gentleman for his kindly expressions. That the grapnel will be some day serviceable for light cables we have no doubt; but we think that it is likely to prove of the greatest service in cases of cables whose iron armouring has become oxidised, and in its application to torpedo work.

The Twelfth Annual General Meeting of the Society was held at the Institution of Civil Engineers, 25 Great George Street, Westminster, on Thursday evening, 13th December, 1883—Mr. Willoughby Smith, President, in the Chair.

The minutes of the Ordinary General Meeting held on November 29th were read and approved, and the names of new candidates were announced and suspended.

Donations to the Library of the Society were announced as having been received from Professor W. E. Ayrton, F.R.S., Col. Frank Bolton, and Mr. J. E. H. Gordon, B.A., and a hearty vote of thanks was awarded those gentlemen for their contributions.

The Secretary read the following:—

REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING OF THE SOCIETY.

December, 1883.

The Council are enabled to report that the Society continues to increase considerably in numbers.

The number of members of each class elected during the year has been as follows:—15 Foreign Members, 10 Members, 123 Associates, and 14 Students.

Besides these, 31 candidates have been approved for ballot next month.

By deaths and resignations our losses are as follows:—1 Hon. Member, 8 Foreign Members, 10 Members, 7 Associates, and 1 Student.

The losses by death have been, unfortunately, unusually severe, not perhaps in point of numbers, but in the large proportion of eminent men among those who have been taken from us.

We have to deplore the loss of our Honorary Member, General Sir Edward Sabine, whose scientific labours, more especially in connection with terrestrial magnetism, are too well known to require enumeration; of Mr. C. V. Walker, Past-President, one of the oldest associates of Messrs. Cooke and Wheatstone in their practical operations, and one of the originators of the Electrical Society, of which this Society is in some respects the successor;

of Mr. Cromwell Fleetwood Varley, well known for his researches in every branch of electrical science, and for his numerous inventions in methods of its application; of Alfred Niaudet and L. Bréguet, distinguished Foreign Members; and within the last few weeks the sudden death of Sir William Siemens, twice our President, has inflicted a grievous loss, not only upon this Society, but upon the whole of the scientific world.*

The Society continues to be indebted to the Institution of Civil Engineers for the great privilege of holding its meetings in the theatre of the Institution.

Of the papers read during the year, of which a list is subjoined, several have proved of great interest, especially Professor Hughes's paper "On the Cause of Magnetism;" but the Council regret that, among those eligible for competition for premiums, they are unable to discern sufficiently marked features to justify their awarding more than the Society's prize of £10 to Mr. J. Munro, Associate, for his paper on a "New Telephone Transmitter." They are unable to award either the Paris Exhibition premium or the Fahie premium for this year; but, nevertheless, desire to make honourable mention of the following papers, viz.:—"The Measurement of Electricity for Commercial Purposes," by Mr. J. N. Shoolbred, Member, as a valuable communication on the subject; and "The Static Induction Telephone," by Mr. W. Moon, Associate, for its originality.

The Council trust that next year they may find themselves justified in awarding all the three premiums offered.

PAPERS READ DURING THE SESSION 1883.

Date.		Title.	Author.
Feb.	8.—	The Magnetic Storm of November 17th, 1882	James Graves.
„	8.—	Simultaneous Effects of Lightning on Different Cables	James Graves.
„	8.—	A Magnetic Storm in India ...	E. O. Walker
„	8.—	Earth Currents in India	E. O. Walker.
„	8.—	Earth Currents, Sun Spots, and Electric Storms	A. J. S. Adams.

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* A memoir of the late Sir William Siemens is being specially prepared for this Journal, and will appear in a future number.

Date.	Title.	Author.
Feb. 22.	—The Measurement of Electricity for Commercial Purposes ...	J. N. Shoolbred, M.Inst.C.E.
Mar. 8.	—Some New Forms of Telephone Transmitters, with a Note on the Action of the Microphone...	J. Munro.
April 12.	—Microphonic Contacts	Shelford Bidwell, M.A., LL.B.
„ 12.	—The Influence of Surface-condensed Gas upon the Action of the Microphone	I. Probert and A. W. Soward.
„ 26.	—Prof. Blyth's Solenoid Galvanometer	A. Jamieson, F.R.S.E.
„ 26.	—The Physical Action of the Microphone	Prof. D. E. Hughes, F.R.S.
„ 26.	—A Static Induction Telephone ...	W. Moon.
May 10.	—The Electric Holophote Course-Indicator, for the Prevention of Collisions at Sea	J. H. A. MacDonald, Q.C.
„ 10.	—Electro-Motors and their government	Profs. W. E. Ayrton, F.R.S., and John Perry.
„ 24.	—The Cause of Evident Magnetism in Iron, Steel, and other Magnetic Metals	Prof. D. E. Hughes, F.R.S.
Nov. 8.	—Volta-electric Induction	Willoughby Smith.
„ 29.	—Submarine Telegraph Cables: their Decay and Renewal ...	S. Trott and F. A. Hamilton.
„ 29.	—Trott and Kingsford's Automatic Grapnel for Submarine Cables and Torpedo Lines	H. Kingsford.
Dec. 13.	—An Instrument for Measuring the the Strength of a Magnetic Field	J. E. H. Gordon, B.A.
„ 13.	—A Method of Calculating the Total Horse-power expended in a Network of Conductors (such, for instance, as a System of Street Mains)	J. E. H. Gordon, B.A.

The Incorporation of the Society under the Companies' Act, which was registered by the Board of Trade in May last, marks an epoch in its history. The Articles of Association, which have taken the place of the former Rules and Regulations, will be found to differ very slightly from the latter, beyond being more explicit and more conveniently classified under the various sections. They were most carefully revised by a Committee of the Council, who enjoyed the benefit of the legal knowledge of Mr. Fletcher Moulton, as one of its members, and the able services of your Honorary Solicitor, Mr. G. L. Bristow, for whose generous devotion of his time to the matter, the Society is deeply indebted.

The question of a Standard Wire Gauge has again occupied the attention of the Council.

Towards the close of last year the Board of Trade invited the opinion of the Society on some modifications proposed to be made in the Scale which they had previously submitted to the Council, and which the latter had approved, accompanying the expression of their approval with a strong recommendation that the value in millimètres of the different sizes should be given in a separate column.

The proposed modifications were in some respects considered objectionable to the Council, and a deputation of its members had an interview with Mr. Chaney, the head of the Standards Department, when the matter was fully discussed, the result being that a Standard Scale has been adopted which meets with the approval of the Council, and they are glad to see that their suggestion as to the metrical value being given, has been acted upon.

The Council deem it their duty to refer to the services of the Committee which they appointed to deal with this matter, and especially to the valuable co-operation of their Past-President, Mr. Latimer Clark, whose labours in connection with the subject for many years past are well known, and have materially conduced to a satisfactory settlement of the question.

The Society has again been called upon to use its influence, as a Body representing electrical science and electrical industry, in connection with the recent International Electrical Exhibition at Vienna.

When invited so to do by one of the Vice-Presidents of the Commission, who specially visited London in the spring for that purpose, the Council at once stated that, owing to many circumstances, it was not probable that any large number of exhibitors from this country would take part in it.

The result proved that they were right in this anticipation; for, notwithstanding the exertions of a Committee appointed by your Council to do all that was practicable in the matter, the British Commission, whom the Government appointed at a very late date,—consisting of Lord Sudeley, Sir William Thomson, the late Sir William Siemens, and Sir Frederick Abel,—found on their arrival at Vienna but a meagre collection of exhibits in the British Section. At their urgent request, the Council succeeded in obtaining from some of the few members of the Society who were accessible at that time of the year,* the loan of several further exhibits of scientific and historic interest, and these, which were exhibited in the name of the Society, together with an important addition made to the collection already sent out by H.M. Postmaster-General, rendered the British Section more worthy of this country.

The Society is much indebted to Herr Leonhardt, Secretary of the Austrian Society of Engineers and Architects, and also Secretary to the Exhibition Commission, who most kindly undertook the office of honorary representative of the Society and agent for the British exhibitors.

It should be mentioned that the British Commissioners most cordially approved and endorsed all that had been done by the Council previous to their appointment by the Government, and evinced their desire to co-operate with the Society by appointing the Society's Secretary, Mr. F. H. Webb, as Secretary to the Commission in London, and Lieutenant Anstruther, R.E., an Associate of this Society, as Secretary in Vienna.

The Library of the Society continues to receive numerous accessions, both by purchase and through the liberality of members and others, as will be seen by the Librarian's Report, appended hereto.

The Society is much indebted to the gentlemen who represent it abroad as Local Honorary Secretaries and Treasurers, and whose kind services materially facilitate communication between our numerous members abroad and the Secretary in London.

The finances of the Society are in a satisfactory position, and have enabled the Council, notwithstanding the expenses connected with the Incorporation of the Society, to sanction an increase in the annual vote for the purchase of books and for binding; and also, as a tentative measure, an increase in the sum devoted to the preparation and translation of "Abstracts and Extracts" published in the Society's Journal. A further amount of £155 10s. has been invested on account of life compositions received during the current year, making the total amount so invested £876 5s.

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* Messrs. Latimer Clark, Russell Crampton, William Groves, Professor Hughes, F. Jacob, Dr. Obach, Robert Sabine, A. Stroh, Siemens Bros., the late Sir William Siemens, and Sir William Thomson.

The following Report from the Librarian was then read:—

4, BROAD SANCTUARY, S.W., LONDON,
10*th December*, 1883.

F. H. WEBB, Esq.,
Secretary,
Society of Telegraph-Engineers
and Electricians.

DEAR SIR,

I have the pleasure to forward you, for the information of the Council, my Fourth Annual Report upon the Library of the Society.

The number of visitors who signed the visitors' book has been greater than in any previous year, as will be seen from the following figures:—

	1881.	1882.	1883.
Members	180	313	403
Non-Members...	158	248	240
Total	338	561	643

This result is very satisfactory, and shows that as the Library is better known its value to those interested in electrical science is becoming more appreciated. We continue to receive a great number of enquiries for information relating to electrical subjects. These enquiries do not always entail very much trouble; but in some cases the information sought is of such a comprehensive character as to render it sometimes difficult to comply with the wishes of the writers, though I am generally able to give the required information, or indicate where it may be found.

The collection of electrical patents has been carefully kept up to date, and all the most recently published specifications are every week placed on the Library table for reference. This important collection has been very much referred to during the year, and has only to be more widely known to be more fully appreciated by the members of the Society and the public.

The following figures give as nearly as possible the number of electrical patents taken out during the last five years:—

1879.	1880.	1881.	1882.	1883.
300.	260.	450.	845.	620.

The total number of English patents taken out during the last two years, to the 7th instant, amount to 11,644, of which no less than 1,465 relate to electricity and its applications, or equal to about 12·6 per cent.

The thanks of the Society are again due to H.M. Commissioners of Patents and to Mr. R. Morris, the Superintendent of the Store Department, for the prompt manner in which the published specifications have been supplied to the Society.

The number of works added to the Library has been far greater than in any previous year, in some measure owing to our having been able to secure a valuable collection of electrical pamphlets formed by our Past-President, the late C. V. Walker. The number of works added to the Library during the last three years, both by presentation and by purchase, will be seen from the following table:—

NUMBER OF WORKS ADDED TO THE LIBRARY.

	1881.	1882.	1883.
By Presentation	156	151	204
„ Purchase	52	94	284
Total	208	245	488

A catalogue of the recent accessions to date will be found appended hereto, other lists having been printed in each number of the Journal of the Society published during the year.

Although the thanks of the Society are due to many members who have presented copies of their works to the Library, I regret that this practice is not so universal as could be desired. These presentations are particularly necessary, not only in order that such works may be properly preserved and catalogued for future reference, but in order that the fund placed at the disposal of the Library Committee for the binding and purchase of books and periodicals may be reserved, as far as possible, for the purchase of recent foreign works, and of such works of reference as should be, but are not in the Library. Many members have expressed a desire that a few standard works on Chemistry and Physics should be added to the Library; and a want has been much felt for some of the old scientific serial publications, such as *Brewster's Edinburgh Journal*, *Nicholson's Journal of Natural Philosophy*, *Silliman's Journal*, *Poggendorff's Annalen*, *Proceedings of the Royal Institution*, *Mechanics' Magazine*, *Repertory of Arts*, *Journal of the Society of Arts*, and many others too numerous to mention. Many of these publications contain electrical papers of the greatest importance; and although something has been done towards making up for the deficiency during the last two years, the amount voted for Library account (a large percentage of which is expended in binding) is too small to allow of the purchase of these serials, many of which are very costly. I would venture to say that we should be glad to place upon the shelves any donations similar to that of Mr. Louis Crossley, who presented to the Library some years ago a complete set of the *Philosophical Transactions of the Royal Society*.

In addition to the periodicals referred to in the list published in Part 47, Vol. XII. of the Society's Journal, several others are regularly received, and our collection of current scientific periodicals is now a very complete one.

During the year I submitted to the Library and Editing Committee a proposal for the compilation of a catalogue of electrical literature, classified into subjects. This proposal was accepted by the Committee, and I have made considerable progress with the work. It is, however, one of great labour, and it will be some time before I shall be able to report its completion.

At the desire of the "Committee of Advice with respect to the International Congress for the Determination of Electrical Units," I prepared a complete catalogue of all the English works relating to Earth Currents. This catalogue, which was printed in the Report of the Committee to the Lords of the Committee of Council on Education, published by the Science and Art Department, is appended hereto.

I represented the Library at the Annual Conference of Librarians, held this year at Liverpool, and was again elected a Member of Council of the Library Association of the United Kingdom.

In conclusion, I beg to say that, with the co-operation of the members of the Society, my efforts shall be devoted in the future, as they have been in the past, towards making the important collection under my charge as useful as it can be to those interested in electrical science.

I am,
Faithfully yours,
A. J. FROST,
Librarian.

The meeting approved of the foregoing Reports being adopted, and printed in the Journal of the Society.

The voting-papers having been deposited, Mr. R. Collett and Mr. W. Kieser were appointed scrutineers.

Professor G. C. Foster: May I be allowed to make a proposition, which is an outcome of the Council's Report, that we should offer to the Institution of Civil Engineers our cordial thanks for the hospitality which they have during the past year continued to us, and which they have extended to the Society since its foundation. I need not remind the Society how much of the success which has attended our meetings since the beginning has been contributed to by the kindness of the Institution of Civil Engineers. I will therefore, without further preface, move the following resolution:—" That the cordial thanks of the Society be presented to the President and Council of the Institution of Civil Engineers for their kindness and liberality in continuing to allow the meetings of the Society to be held in their Lecture Hall."

Col. E. D. **Malcolm**, C.B., R.E., seconded the proposition, which was heartily adopted by the meeting.

Mr. E. **Graves**: There is one gentleman whose special claims to the thanks of the Society eminently deserve our recognition and acknowledgment—I refer to our Honorary Solicitor, Mr. G. Bristow. From the foundation of the Society his time and trouble have always been at our service, and he has never spared himself any exertions that were requisite to give us the benefit of his advice in matters that concerned his profession. In fact, he has been throughout our legal guide, philosopher, and friend. During the past year, however, a special call has been made upon his exertions: he has been particularly engaged in our interests in effecting our metamorphosis from a voluntary association, without any legal status, to a fully registered Limited Liability Company. With the assistance of Mr. Moulton, our legal Member of Council, many important and engrossing duties in connection with the task have been performed; and it is specially noteworthy that the work has been done purely in the interest of the affairs of the Society, no expense thereby arising excepting the unavoidable charge for stamps and things of that kind. Therefore it is unnecessary for me to use many words to invite you to accept the proposition which has been entrusted to me—"That the thanks of the Society are due to Mr. Bristow for his valuable services in connection with the incorporation of the Society."

Seconded by Mr. Hy. **Edmunds**, Jun., and carried unanimously.

The **President**: I also have a pleasing duty to perform in asking you to give your cordial thanks to our Honorary Secretaries and Treasurers abroad. No one knows better than the Members of the Council how those gentlemen, one and all, labour for the benefit of the Society, and they certainly deserve our best thanks. I therefore propose—"That the thanks of this Society be given to those gentlemen who have represented the Society abroad as Local Honorary Secretaries and Treasurers during the past year, for their kind attention to the interests of the Society."

The proposition was heartily agreed to.

Mr. C. E. **Spagnoletti**: I have very much pleasure indeed in proposing the next vote of thanks. "Good wine needs no bush."

and therefore I am sure that the vote of thanks I am about to propose will need no words of commendation from me. The proposition is a vote of thanks to our Honorary Treasurer. As a member of the Finance Committee, I may say that I have had the opportunity on several occasions of seeing the very careful and studious attention he pays to the accounts of the Society, which are most carefully and properly scrutinised by him. I have now the pleasure of moving—"That a vote of thanks of the Society are due to Mr. E. Graves, Honorary Treasurer, for his watchful care over the finances of the Society."

Major R. Y. **Armstrong**, R.E., seconded the motion, which was carried unanimously.

Mr. E. **Graves**: I am very glad, gentlemen, that the management of the finances of the Society meets with your approval. My own share in it has been very little; but I am glad to say that, since I took the office of your Treasurer, we have passed from a state which I may call insolvency, to that of investors of capital, and that fact in itself is the most satisfactory answer I can give to the vote of thanks you have been kind enough to give me.

The following paper was then read:—

ON AN INSTRUMENT FOR MEASURING THE STRENGTH OF A MAGNETIC FIELD.

By Mr. J. E. H. Gordon, B.A.

In the construction of dynamo machines, it is extremely important to know the true field that our magnets produce. We do not, when we have got a general idea before us of a new dynamo, want to spend the money, and, perhaps what is worse, the time, required to complete the dynamo before we have any notion of what the strength of the field is. It is a very great advantage if we can construct one single magnet of the size that we think is about right, and then try what its field is. There are various ways of measuring magnetic fields, including the ordinary way of measuring the earth's horizontal magnetic field, which consists in vibrating a small magnet; but none of them are quite satisfactory for the very powerful field produced in dynamo machines, because, if the magnet happens to stick for an instant, the magnetism of the magnet of the dynamo is so strong that it destroys or reverses the magnet; in fact, if the iron cannot turn round, it turns the magnet round without it. For that reason it is necessary to have some instrument that will measure magnetic fields in a manner that will be quite independent of disturbing causes, and I have here an instrument which is based on one invented by M. Verdet, the French physicist, and used by him in his experiments on magnetism and polarised light. I have used this instrument a good deal, and have found it a very fairly satisfactory instrument. It is really a very minute dynamo machine which turns in the field, but it only makes one quarter-turn. If a small coil of wire is moved through a quarter-turn in a magnetic field, the total number of coulombs of electricity generated in it depends merely on the magnetic field, and for a given coil and resistance upon nothing else, being independent of the speed of motion. If the motion is made rapidly then, if the terminals of the coil of wire are connected to a suitable galvanometer, the needle will make a sudden swing, and the sine of half the angle of swing is proportional to the total quantity of electricity generated—that is, is proportional to the strength of the magnetic field. If we use a reflecting galvanometer, shunted down properly so as to keep the deflection extremely small, then the total quantity of electricity will be measured, not by the sine of half the swing, but practically by the swing itself. Therefore, if we can turn a small bobbin of wire rapidly and suddenly a quarter-turn in a magnetic field, and connect it to a reflecting galvanometer at a sufficient distance to be quite undisturbed by the magnetic field, we shall get an accurate measurement of the strength of that field. Of course that is not a measure in absolute units, but we can reduce it to absolute units, if required, by causing the same instrument to move under the influence of the earth's magnetism, whose strength we know, by the ordinary methods; or, what is more important, if we have a dynamo of a certain size, we can put side by side two magnets, one of which is exactly similar to that in the working dynamo and the other is the magnet of the proposed dynamo, and can so compare the fields produced by them. Also, by such an instrument we can find out how soon the magnets become saturated, by sending currents of varying intensity through the coils of the magnets and taking the field step by step corresponding to those currents, and so draw a curve showing the relation of the magnetic field to the strength of the current.

The instrument consists of a very small coil of wire, which turns through 90° round an axis at right angles to its own axis. In working, contact is broken, then the coil turned 90° by hand; then contact is restored, and, the handle being released, the coil flies back through the 90°, under the pressure of a spring, when immediately the needle gives a swing, and the limit of that swing is an exact measure of the strength of the field, or at least it is quite exact enough for all practical purposes. This instrument serves not only to measure the strength of the field at one point, but to make a complete chart or map of the field, and to map out the whole of the lines of force; and it is in the method of doing this that this instrument differs from the old one of M. Verdet. The vertical height above the magnetic pole is regulated by sliding the coil up and down on the stem, and is indicated by a scale of inches graduated on the stem.

[The various ways of setting the instrument for obtaining different observations were explained to the meeting by Mr. Gordon.]

The instrument is made to project over the end of the base, and the reason of that is, because we often want to see how much the magnetic field will be disturbed by the iron core of the armature; and in order to do that the iron core of the armature is put into position when the observation is taken, and the projecting piece of the instrument enables the effect of its presence to be observed.

This instrument can be used for two different purposes. Firstly, by its means the strength of magnets of different sizes can be compared. The best way to do that is to wind their bobbins with wire of the same gauge, and then connect the two magnets in series, and put one of Professor Ayrton's, or other absolute galvanometer, in circuit. Then we shall know what current we have, and we know that we are expending the same horse-power per cubic inch of the wire space in magnetising both magnets. When we have determined the relative strength of our magnets, we know that the magnet we have been experimenting on will have exactly the same strength when the same horse-power has been expended in magnetising it, even if we wound it of an entirely different gauge and used a different electro-motive force, as long as the product of the electro-motive force and current remains the same.

Secondly, we can see to what extent it is advantageous to increase the magnetising currents in a magnet, because, as you all know, the time of saturation comes when even a considerable increase in the magnetising current does not make any appreciable further increase in the magnetic field, and of course long before that is the time to stop increasing the current in the magnets of dynamo-machines.

The PRESIDENT: Before we discuss what Mr. Gordon has just said, we will take his second paper, and discuss the two together.

The scrutineers, however, advise me that the result of the ballot for Officers for the coming year is as follows, so I will announce the results first:—

President.

Professor W. G. ADAMS, F.R.S.

Vice-Presidents.

C. E. SPAGNOLETTI.
Professor D. E. HUGHES, F.R.S.
Sir CHARLES BRIGHT, M.Inst. C.E.
EDWARD GRAVES.

Members of Council.

W. T. ANSELL.
Professor W. E. AYRTON, F.R.S.
E. B. BRIGHT.
T. R. CRAMPTON, M. Inst. C.E.
R. E. CROMPTON.
W. CROOKES, F.R.S.
H. C. FORDE, M. Inst. C.E.
J. E. H. GORDON, B.A.
Dr. J. H. HOPKINSON, M.A., F.R.S.
Col. E. D. MALCOLM, R.E., C.B.
ALEXANDER SIEMENS.
AUGUSTUS STROH.

Associate Members of Council.

Capt. G. W. ADDISON, R.E. | GEO. CHATTERTON.
MARTIN F. ROBERTS, F.C.S.

Honorary Treasurer.

EDWARD GRAVES.

Honorary Secretary.

Colonel FRANK BOLTON.

Auditors.

J. WAGSTAFF BLUNDELL. | FRED. C. DANVERS.

Honorary Solicitors.

WILSON, BRISTOWS, & CARPMAEL.

A vote of thanks was passed to Messrs. R. Collett and W. Kieser for acting as scrutineers of the ballot.

The following paper was then read:—

ON A METHOD OF CALCULATING THE TOTAL HORSE-POWER EXPENDED IN A NETWORK OF CONDUCTORS (SUCH, FOR INSTANCE, AS A SYSTEM OF STREET MAINS).

By J. E. H. GORDON, B.A., Member.

In all systems of electric lighting it is important to know what proportion of the electricity generated is utilised in the lamps, and what proportion is wasted in heating the conductor.

When we have a rule for determining this we can properly apportion the diameter of each conductor to the current it has to carry, and to the distance to which it has to carry it; so that, on the one hand, we may not, by making the conductor too small, expend too great a quantity of coal in forcing the current through it, nor, on the other hand, by making it too large, so increase our capital expenditure on copper that the interest on it is too large a proportion of the annual rental which we can charge for the electricity used in the lamps.

When the *same current* passes through two resistances, such, for instance, as a wire and the lamps fed by it, the horse-powers expended in the two resistances respectively is simply proportional to the resistances. For, if r and r' are the two resistances, the horse-powers expended in them by the *same current* (C) are respectively,

$$\text{H.P.} = \frac{C^2\, r}{746},$$

and

$$\text{H.P.}' = \frac{C^2\, r'}{746},$$

and their ratios are

$$\frac{\text{H.P.}}{\text{H.P.}'} = \frac{\dfrac{C^2\, r}{746}}{\dfrac{C^2\, r'}{746}} = \frac{r}{r'} \qquad \ldots \qquad \ldots \quad (1)$$

When, as in arc-lighting, the lamps are all placed in series, the determination of the relative horse-powers is very simple, for the wire is of uniform section throughout, and its total resistance is its resistance per yard multiplied by its length in yards.

The resistance of each lamp is known, and the total lamp-resistance is the sum of these resistances.

Example.—Sixteen arc lamps, each of 2·1 ohms resistance, are placed on a circuit of 450 yards long, consisting of a wire having a resistance of ·006 ohm per yard. What proportion of the horse-power is used and wasted respectively?

The lamp-resistance $r = 16 \times 2{\cdot}1 = 33{\cdot}6$ ohms.

The wire-resistance $r' = 450 \times {\cdot}006 = 2{\cdot}7$ ohms.

The ratio

$$\frac{\text{H.P.}}{\text{H.P.}} = \frac{r'}{r} = \frac{2{\cdot}7}{33{\cdot}6} = {\cdot}08,$$

or 8 per cent.

Note.—We must be careful not to confuse the ratio of horse-power wasted to horse-power used with the ratio of horse-power wasted to total horse-power.

The latter is the ratio of wasted horse-power to the sum of the wasted and used horse-powers, or

$$\frac{\text{H.P.}'}{\text{H.P.} + \text{H.P.}'} \qquad \ldots \qquad \ldots \qquad \ldots \quad (2)$$

This, in the case when the current is the same throughout the circuit, still depends only the resistances and is given by the formula

$$\frac{\text{H.P.}'}{\text{H.P.}+\text{H.P.}'}=\frac{r'}{r+r'} \quad \dots \quad \dots \quad (3)$$

With a circuit as given in the previous example, the ratio of the horse-power wasted to the total horse-power would be

$$\frac{2{\cdot}7}{33{\cdot}6+2{\cdot}7}={\cdot}074, \text{ or } 7{\cdot}4 \text{ per cent.}$$

With one group of incandescent lamps, either in quantity series, or of any combination of the two placed at the end of a pair of leads, as in Fig. 1, the problem of the determination of the relative horse-powers wasted and used is equally simple; for

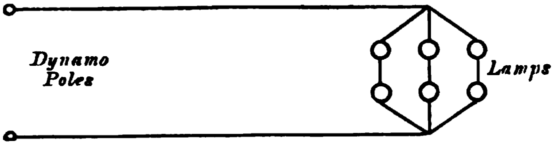

FIG. 1.

the wire being of uniform section, we know its resistance, and the resistance (R) of the group of lamps is given by the usual formula

$$R=\frac{r\,s}{q},$$

where r is the resistance of one lamp, s the number of lamps in series in each branch, and q the number of branches arranged in quantity.

The current in every part of the leads being the same as the current in the group of lamps, the relative horse-powers are still proportional to the relative resistances.

We see in all these problems that the longer the conductor is, the thicker it must be; for if a given conductor wastes a certain horse-power, and we wish to double its length, *i.e.*, to put the lamps twice as far from the machine, without increasing the waste, we must also double its sectional area, so as to keep the resistance constant.

In practical incandescent lighting, however, the lamps are distributed at intervals along the pair of conductors, as in Fig. 2, and the problem at once becomes much more complex, because different parts of the conductors are carrying currents of different strengths, and the simple formula (1) is no longer applicable.

For in Fig. 2, if we consider the branches L_1, L_2, etc., as single lamps,

the portion *a* of the conductor carries the current of 1 lamp.
" *b* " " 2 lamps.
" *c* " " 3 "
" *d* " " 4 "

If we consider the conductors $+ d\ a$ and $- d\ a$ in Fig. 2 to be the wires laid along a side street, then the branches L_1 L_2, etc., will not be single lamps, but may each be considered to represent the whole group of lamps in one house; while, if we consider the conductors to be the mains in a principal street, the branches L_1 L_2, etc., may be considered as representing the sub-mains branching into the side streets.

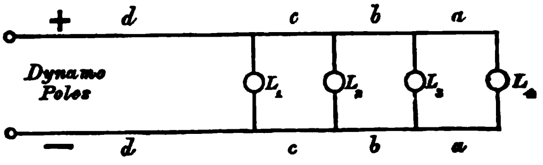

FIG. 2.

We see that L_1 L_2, etc., are not necessarily equal to one another.

In order to determine the relative horse-powers used and wasted in a system of town supply, I prefer to use the following method :—

We first mark out on a large-scale map of the district the number of lamps likely to be required in each block or house.

We then draw the street-mains and branches radiating from the engine-house to the houses to be lighted.

We then, starting from the furthest points on each branch, work up towards the engine-house, marking on each branch and main the number of lamps it has to carry.

In a district so marked out we may use plain numbers to represent the number of lamps* in the block or house on which they are marked, and numbers surrounded by a circle for the number of lamps whose current is carried by the wire near which they are written.

To avoid confusion, the + conductor only is shown, and when the H.P. wasted in it has been obtained, the result must be doubled to obtain the total waste in the + and − conductors. Knowing the current used per lamp, we know the number of ampères which each wire has to carry.

We note that the "carrying" number in each branch is the sum of all the numbers beyond it, *i.e.*, on the side furthest from the dynamo in that branch.

In order to secure the greatest economy of copper and of coals, the section of the conductor must be directly proportional to the current it has to carry, i.e., *as we leave the dynamo, the section of conductor must diminish after each branch leaves it, in order that the same number of ampères per square inch may be carried by every part of the conductors throughout the system.*

This condition being given, then, for a given district mapped out, the amount of H.P. wasted in the conductors is simply proportional to the number of ampères per square inch which we use.

By the method we are about to explain, it is easy to calculate the amount of horse-power wasted in the whole district for any given number of ampères per square inch.

In order to find the ampères per square inch corresponding to the particular percentage horse-power that we are prepared to waste, we must assume some number of ampères arbitrarily, and find the actual horse-power wasted, and then the required number of ampères will bear the same ratio to the required horse-power that the arbitrarily-assumed number does to the horse-power corresponding to it. ... ... (4)

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[*] The number of lamps need not be quite the total number erected, but should be the total number likely to be ordinarily in use. It is not necessary to provide copper to be always in position for lamps that are only lighted occasionally. In putting on extra lamps for short periods, care must, however, be taken that the heating limit is not approached.

For example, suppose in a system where, say, 100 H.P. is being used in lamps, we are prepared to waste $12\frac{1}{2}$ H.P., and that with an assumed current of 500 ampères per square inch we find, by the method of calculation which we are about to give, that we waste 16 H.P., then the right number of ampères per square inch for us to use is

$$\frac{12\frac{1}{2}}{16} \times 500 = 390 \text{ ampères per square inch};$$

and, if we have calculated the section of copper on the basis of 500 ampères per square inch, we must increase that section in the ratio of 16 to $12\frac{1}{2}$.

Calculation of H.P. Wasted.

We now come to the method of calculating the horse-power wasted in a system of conductors when a current of a certain number of ampères per square inch is flowing through it.

When there are the same number of ampères per square inch in a system, the *horse-power wasted in each cubic inch of copper is the same throughout the whole system or district.*

The resistance between the two faces of an inch cube of copper is ·0000007 (seven ten-millionths) of an ohm.

The horse-power (which we will call $\overline{\text{H.P.}}$) expended in a cubic inch of copper with a current of $\overline{C}$ ampères per square inch, is

$$\overline{\text{H.P.}} = \frac{\overline{C}^2 \times \cdot 0000007}{746} \quad \ldots \quad \ldots \quad (5)$$

With 500 ampères per square inch, the horse-power per cubic inch is

$$\overline{\text{H.P.}} = \frac{500^2 \times \cdot 0000007}{746} = \cdot 000234 \quad \ldots \quad (6)$$

When we know this constant, $\overline{\text{H.P.}}$, and also the total number of cubic inches of copper in the district, we know the total horse-power wasted in the district.

To determine the number of cubic inches of copper in the district, we return to our map, on which the number of lamps in each branch is marked, and we calculate a constant for the area of copper per lamp corresponding to our assumed current of, say, 500 ampères per square inch.

For instance, if each lamp takes ·85 ampère, then for each lamp we must have

$$\frac{\cdot 85}{500} = \cdot 0017 \text{ square inch of copper.}$$

We then multiply the number of lamps on each section of the branch or main by this new constant, and we get the required area of this section, which we can mark on it on the map.

We next multiply the area of each section by its length in inches, and this gives us its volume in cubic inches.

Adding all the results thus obtained together, we get the total number of cubic inches of copper in the positive leads throughout the district.

Twice this result is the total amount in the + and − leads together.

Multiplying this result by $\overline{\text{H.P.}}$, the horse-power constant, (which, when $\overline{C} = 500$, is equal to ·000234), we get the total horse-power wasted in the copper in the whole system.

If the H.P. wasted is more or less than the desired amount, we, as we said before, alter $\overline{C}$ proportionately to the desired change in the H.P. wasted.

We of course know the H.P. expended in the lamps, as we know the number of lamps and the H.P. expended in each.

If H.P._{L} is the total H.P. used in the lamps, and H.P._{w} the total H.P. wasted, then the percentage P_{w} of the whole H.P. expended which is wasted in the leads is

$$P_{w} = \frac{100 \text{ H.P.}_{w}}{\text{H.P.}_{L} + \text{H.P.}_{w}} \quad \cdots \quad \cdots \quad (7)$$

As far as we have yet gone, we have assumed that all the lamps are alight whenever the current is flowing.

In practice this will not be the case, and we must note that if, the conductors remaining unchanged, we diminish the number of lamps on every branch in a certain *uniform* ratio, we shall diminish the wasted H.P. *in the square of that ratio.*

That is, if when 1,000 lamps are burning we are using 100 H.P. and wasting 10 H.P., then, if we reduce the number of lamps to 500, we shall reduce the used H.P. in the ratio of $\frac{500}{1000}$, *i.e.*, to 50 H.P., but we shall reduce the wasted H.P. in the ratio of $(\frac{500}{1000})^2$, or to $\frac{1}{4}$ of its former amount, namely, to $2\frac{1}{2}$ H.P.

To put this in symbolical form, we may say that, with a given system of conductors, if

H.P._{WM} is the H.P. wasted when M lamps are burning,

and

H.P._{WN} „ „ N „ „

then

$$\text{H.P.}_{WN} = \left(\frac{N}{M}\right)^2 \text{H.P.}_{WM} \quad \dots \quad \dots \quad \dots \quad \dots \qquad (8)$$

This formula is, as we said above, only correct when the number of lamps diminishes uniformly over the whole system, *i.e.*, when an equal proportion of the lamps in every block are turned out simultaneously.

In districts containing the same class of houses, the condition is sufficiently nearly approximated to in practice to make the formula (8) a useful one in calculating probable waste. Assuming, then, this condition, we can, if we know the general average habits of the district as to the use of light, calculate the total relative quantities of coals which will be used in the engines in producing useful and wasted electricity respectively.

We will use the symbol H.P.H. for "horse-power-hour," *i.e.*, for a H.P. working for an hour. Thus, 20 H.P. working for 3 hours would be equal to 60 H.P.H.

The coals used in an engine are practically proportional to the H.P.H., *i.e.*, to the H.P. developed, multiplied by the hours during which the engine works. In order to determine the ratio of the coals used in producing wasted and useful electricity, we must take the H.P.H. used and wasted hour by hour throughout the night.

This will be best understood by an example.

Suppose that we have 1,000 lamps and a given system of mains, such that, when all the lamps are on, we use 100 H.P. and waste 10 H.P., and suppose that the number of lamps in use at the different parts of the night are as in the first two columns of the following table, then the H.P.H.'s used and wasted will be as in the fifth and sixth columns respectively, where the letters L and W stand for "used in lamps" and "wasted" respectively.

Hours.	Lamps burning.	H.P._L	H.P._W	H.P.H._L	H.P.H._W
Before 5 p.m.	Very few	Inappreciable	Inappreciable		
5—6 „	100	10	·1	10	·1
6—7 „	500	50	2·5	50	2·5
7—10 „	1,000	100	10·0	300	30·0
10—11 „	800	80	6·4	80	6·4
11—12 „	400	40	1·6	40	1·6
12—2 a.m.	200	20	·4	40	·8
After 2 „	Very few	Inappreciable	Inappreciable		
			Total	520	41·4

Thus, although the percentage H.P. wasted when all the lamps are on is from (7),

$$P._W = 100 \frac{10}{100 + 10} = 9{\cdot}9 \text{ per cent.},$$

yet the percentage of coals wasted in the whole night is only

$$P._W = 100 \frac{41{\cdot}4}{520 + 41{\cdot}4} = 7{\cdot}3 \text{ per cent.}$$

The **President**: The subject brought before us by Mr. Gordon is at the present time a very important one, but it is one with which I am not directly connected, therefore I will not occupy or waste your time with any remarks of my own. I see many gentlemen here present who are more competent to discuss the question than I am, and I now beg of them to give us their opinions on what we have heard to-night.

Mr. R. E. **Crompton** said that such a mathematical and interesting paper could not be fairly criticised without full consideration. The first noticeable feature in the paper was Mr. Gordon's assertion that, "in order to secure the greatest economy of copper, as well as of fuel consumption, the section of the conductor must be directly proportional to the current it has to carry, *i.e.*, as we get more distant from the dynamo the section of the conductor must diminish after each branch leaves it, in order that the same number of ampères per square inch may be carried by every part of the conductors throughout the system."

In fact, Mr. Gordon supposed the system of conductors to be like a tree branching out with tapering limbs, the fine twigs representing the extremities. He was afraid that such a state of things would never be found in real practice, but that the conductors must in most cases form a network extending over the whole of the district to be supplied with electricity. Again, other network systems must be connected on to it which, if Mr. Gordon's tree-like form were adopted, would require the connections between the two networks to be through the fine twigs, a result which would not be satisfactory.

These conditions rendered it necessary to keep the section of the conductor constant, right up to the end of the twigs, in fact, throughout the whole network, and this necessity altered the weight of copper from that calculated for by Mr. Gordon so enormously as to make his rule of small practical service.

Again, the whole of the calculations made by Mr. Gordon were based on two suppositions, *first*, that one simple network would be employed; *second*, that the weight of copper to be used in the mains depended upon the amount of horse-power which could be afforded to be wasted in the mains. Whereas, those who had been looking into this question found that the real consideration governing the weight of copper was that of keeping the potential or E.M.F. in the mains within very narrow limits. These limits had been fixed by the Board of Trade at 5 per cent. on either side of the standard E.M.F.; but actual practice showed that even this variation caused far too great variation in the amount of light given by an incandescent light worked up to an economical temperature of filament. Experiments showed that from 2 to 2½ per cent. on either side of the standard would probably be the utmost variation which would be tolerated by the users of the electricity. In order to maintain the E.M.F. in the mains at or near the houses within these very narrow limits, the simple tree-like form would have to be replaced by a far more complex system.

It appeared probable that the houses must be connected on to a network extending over the whole area. The section of the conductor forming the network must be constant. His own investigations led him to suppose that a comparatively light section of main in the network, fed at many points by feeders of light section, as compared with the current they would have to carry (at times of excessive demand), would be the method most likely to utilise the copper in the mains to best advantage. In laying out such a system of mains, the section of conductor of the network would depend on the number and distance apart of the points at which the feeder-mains are attached to the network, and must be calculated on the supposition that the extreme fall in E.M.F. from these feeding points to the lamps in the house must not exceed the above-mentioned limits. The section of the feeder-mains might be calculated by some modification of Sir Wm. Thomson's rule, which would take into consideration on the one side the interest, etc., on the capital charge of the first cost of mains, including copper, insulation, and roadwork; and on the other side, the cost of the loss in horse-power during the comparatively short time during which the feeders would have heavy currents passing through them.

As he had said before, to do these matters full justice requires more time and consideration than could possibly be given to it in the general discussion.

He noticed the term "horse-power hours" used by Mr. Gordon, and suggested that it would be much more convenient to adopt for all such calculations the official "unit" of 1,000 volt-ampères flowing for one hour, adopted by the Board of Trade for all the electric lighting provisional orders. This unit is about equal to 1¼ horse-power for an hour, and as it is to be the legal measure of electricity it was as well to become at once accustomed to its use.

Professor W. E. AYRTON: Mr. Crompton has put the matter very clearly. There are two totally different points to be taken into consideration in deciding on the size of the conductor to be used for electric-lighting mains—one, the question of the horse-power wasted in the conductor; the other (and a much more important one), the necessity for arranging the conductors in such a way that the potential difference shall not differ seriously from house to house, or, what is still more important, shall not differ seriously in the same house from hour to hour.

Five per cent. difference, allowed in the Provisional Orders, is far too great; four per cent. I find also too great, for with that variation of the potential in the terminals, I find lights go up and down in a most serious and annoying way.

The question of the horse-power expended appears to me to have been fully dealt with by Sir William Thomson in his communication to the British Association in 1881, in which he considered very carefully as to what should be the proper section to give to a conductor. He showed that if you were only considering the question of the power wasted in the conductor, and not going into the question of difference of potentials at different points, but merely, as I have said, considering the power wasted in transmitting electricity for a certain distance, then that the section of conductor ought to be directly proportional to the current, and independent of the length. And further, that taking as £10 per annnm the commercial value of a horse-power in England, and the value of good conductivity copper as something like £70 per ton, he arrived at the conclusion that if you want the waste of horse-power in heating the wire, *plus* the waste of interest on capital expended in the copper lying, so to say, useless in the street, then you ought to put about (if I remember rightly) one square centimètre of copper for every 50 ampères, which I think comes to about a square inch for every 250 ampères. That calculation has, of course, nothing whatever to do with the question of safety. His conclusion, mathematically arrived at, leads also to the result that the value of the horse-power wasted in heating the conductor must be equal to the interest on capital expended.

Mr. Crompton most properly drew attention to the very important question of arranging the conductors so that there shall not be more than but a very limited amount of fluctuation of potential difference at the terminals of the house. That is an extremely complicated matter, and cannot be settled by any easy rule. Thomson's rule would apply if the generator was at a given point, and the first set of houses at, say, a mile away, and would show the money required in copper, disregarding the sheathing of the copper—it would tell you the proper diameter to give to your conductor; and then comes the very serious question as to how the houses are to be joined on to this main conductor.

As regards the very ingenious apparatus which Mr. Gordon has brought before us for measuring the strength of a magnetic field, I would feel obliged if he will kindly tell us how the measurement is to be taken when we want to know the strength of the magnetic field between the iron of the armature and the iron of the field-magnet. The space is too small to insert this instrument; and one cannot remove, say, the iron of the armature and measure the field produced by the field-magnet near the place previously occupied by the iron of the armature, and thus presume that the new field is the same as the old one which you wished to measure. The iron of the armature not only strengthens the field, but completely modifies the direction of the lines of force. Take, for example, a Gramme, or Edison, or Siemens machine, then if the iron of the armature were removed the field would be very weak, owing to the projections of the pole-pieces of the field-magnet leading the lines of force away from the space which is usually occupied by the armature. In fact, measurements made without the armature in position would give no indication of what the field was when the armature was there. But if the armature is in position, it is difficult to get a piece of apparatus like that of Mr. Gordon's between the iron of the armature and that of the field-magnet, and consequently an extremely thin coil of wire has been adopted by other experimenters for the purpose; the ends of such a coil are joined to a galvanometer, and then, the thin flat coil having been put at the spot where we desire to measure the strength of the magnetic field, it is suddenly jerked away, and the galvanometer deflection gives us what we desire. Any field measurer, to be of real service, must be able to be put in the place where you want to measure the strength of a magnetic field, and must not, in order to be used, necessitate removal of the mass of iron which is there when you are using the machine, and which most seriously alters both the distribution and number of lines of force.

Professor G. C. **Foster** asked Mr. Gordon why his instrument for measuring the strength of a magnetic field moved through a quarter-revolution, instead of a half-revolution, which at first sight would appear to be better, because the movement must either begin or end at a point of maximum induction, and a small error in the amount of motion (if the motion were a little more or less than an exact quarter-revolution) would cause an error in the measurement obtained. But if the rotation were through two right angles, starting from a position of minimum action and passing to another position of minimum action, any little error at the starting or ending would have scarcely any influence on the measurement.

As to the use of a shunt mentioned by Mr. Gordon, the Society would remember a paper, read in one of the early years of its meetings, by Mr. Latimer Clark, who pointed out what was very well known to electricians, that although a shunted galvanometer may give an accurate measurement for a steady current, it was not a thing to be used without consideration in the case of an instantaneous current: the shunt-ratio was different in that case. It would appear preferable to reduce the throw of the galvanometer by putting in resistance rather than by shunting in such a case.

Professor D. E. HUGHES had as yet no opportunity of experimenting upon the powerful electro-magnet of dynamo machines, but in the course of his investigations upon the cause of magnetism he often needed some method of reading the amount of evident magnetism on different parts of the same bar or sheet of iron or steel.

He had shown in his late paper upon the theory of magnetism, that if soft iron had mechanical vibrations given to it, such as blows from a wooden mallet, or slight bending to and fro, that the molecules were allowed far greater freedom during this vibration, and rotated in accordance with the exterior directing force, the force observed on the rod being in proportion to the strength of the exterior field. Now, by placing a soft iron wire near or in contact with different parts of a magnet, vibrating it whilst in this position, we could remove the rod and take measures of the amount of evident magnetism induced in the rod by its contact at any given spot. The rod should be rendered neutral before each trial by vibrating it in a neutral field, or east and west. We can thus easily map out the distribution of evident magnetism on small bars and sheets, the operation being very similar to the "proof plane" employed many years since for static electricity.

He could not say that this method would have any value for the purpose for which Mr. Gordon had designed his admirable instrument. The instrument of Mr. Gordon he regarded as being designed for the special conditions which he had mentioned, and for this he thought it to be perfectly adapted.

Mr. **Gisbert Kapp** asked whether it was a matter of importance what position the axle of the small dynamo occupied in relation to the edge of the pole-pieces, or to the force of it. If an error were made in setting the instrument to the correct angle it might make the observations erroneous.

Mr. **Thomas Blakesley** thought that some confusion existed as to the real meaning of the law of economy ruling the dimensions of conductors, and he hoped he could put the matter in a somewhat clearer light. Mr. Gordon gave the law as having merely to do with electrical quantities, whereas it involved pounds, shillings, and pence in the cost of materials and power. Sir William Thomson's law was only true of a conductor the cost of which varied as its cross-section, but every one knew that there were elements of expense in installing a conductor which did not vary in this way. Some items, indeed, would not change with the dimensions of the conductor at all, *e.g.*, the rent of a subway, within limits. Other items would vary merely as the diameter of the conductor. In a land-cable well insulated and covered with gutta percha and jute, it will be found that the expense representsing this covering (*i.e.*, the whole expense less the expense of the bare conductor) varies nearly as the diameter of the conductor, and is often the most considerable part of the expense. Again, the cost of galvanising should vary as the diameter, or, if a cable were sheathed in a mantle of lead of a certain fixed thickness, so as to keep the capacity of the cable the same, then the cost of this lead would be as the diameter of the conductor.

Now, in taking these matters into consideration when fixing the size of the most economical conductor,—in the first place, from the whole expense of the installation must be deducted entirely those items which are independent of the size of the conductor; secondly, a further deduction must be made of half the expenses, which vary as the diameter or square root of the cross-section. Then the interest on the remainder should be equal to the yearly cost of the energy absorbed in the conductor, when the economical dimension is attained. He gave the following proof of the propositions:—

Let the yearly interest on a conductor be composed of three terms—

1st, a, a constant term;

2nd, $b\,A^{\frac{1}{2}}$, a term varying as the diameter;

3rd, $c\,A$, a term varying as the area of cross-section,

where a, b, c are constants, and A is the area of cross-section. Let the expense of absorption in the same time be represented by the term $d\,\frac{C^2\,t}{A}$, where d is a constant, C is the current, t the time during which it is employed. Then the whole expense, E, is obviously the sum of these four terms—that is,

$$E = a + b\,A^{\frac{1}{2}} + c\,A + \frac{d\,C^2\,t}{A},$$

and this has to be made a minimum, a condition determined by the equation

$$\tfrac{1}{2}\,\frac{b}{A^{\frac{1}{2}}} + c = \frac{d\,.\,C^2\,t}{A^2}.$$

The inspection of this shows that the current is not proportional to the cross-section, except in the rare cases where b is zero or negligible. If the above equation is multiplied through by A, it becomes—

$$\tfrac{1}{2}\,b\,A^{\frac{1}{2}} + c\,A = \frac{d\,.\,C^2\,t}{A}.$$

The first term on the left is half the expense varying as the diameter; the second term is the whole expense varying as the cross-section. The constant term a has disappeared, and the term on the right is the yearly expense of absorption; which proves the propositions stated.

Professor W. E. AYRTON was obliged to the last speaker for drawing attention to an apparent omission on his part in reference to Sir William Thomson's law. In his remarks, however, he was not drawing attention to what might occur in practice, but simply to Sir William Thomson's law. As a matter of fact, when his colleagues and himself were making calculations last year for underground wires on a very large scale, in Paris, he had to take into account, not merely the cost of copper, but of iron pipes, paraffin oil, and jute, which was the material proposed for insulating the wire, and of course, as Mr. Blakesley had said, things which were not proportional to the sectional area of the copper had to be considered, and the result would not be that of Sir William Thomson's law. Sir William Thomson's law was given for overhead wires between Niagara and New York, and would apply, because in that case the cost of the wire was very much greater than the cost of the insulation. In small installations, even where bare wires and insulators were used (and he knew of a considerable installation that was now being made in a large public building), Sir William Thomson's law might also not apply, because the labour of erecting wire was a serious matter, and was not taken into account in Sir William Thomson's equations.

Mr. C. A. **Carus Wilson** said it would be very interesting to have some further explanation as to what Mr. Gordon meant when he said: "We then, starting from the furthest points on each branch, work up towards the engine-house, *marking on each branch and main the number of lamps it has to carry.*" That seemed to him the greatest difficulty in the whole arrangement. With a single street it was easy to find the total current it would have to carry; but the difficulty arises when we come to a network. To determine what current each street of a network should carry is a very laborious matter to work out arithmetically. Some graphic method might perhaps be possible: Mr. Gordon could perhaps state how he proposed to do it.

Mr. J. E. H. **Gordon**, in reply, said: As to the advantage of a half-turn instead of a quarter-turn, suggested by Professor Foster, no doubt, as the instrument now stands before you, a half-turn would be more advantageous; but we very often want to know the magnetic force when the coil is close down on the magnet pole, and in such a position there would not be room for the lever to work. As it is, we can only get within three-quarters of an inch of the pole, and a half-turn arrangement would pull us up an inch more, while the bottom inch is a very important part to investigate, and in designing the instrument, I thought it would be well to sacrifice the accuracy to be obtained by half a turn in order to be able to get quite close to the pole.

With regard to the shunted galvanometer, if I compared a strong magnet with a weak one, I should not propose to work the galvanometer without any shunts with the latter and put in shunts with the former: I am quite aware that that would not give a proper comparison. With the same shunt in throughout the whole experiment we take the field proportionate to the swing, and that is the way we always work in practice. I should never think of using different shunts in two experiments which are comparable to each other; but in spite of the necessity of using shunts which it involves, I prefer an instrument where the strength of the field is proportionate to the deflection instead of to the sine of half the angle, which would be too complex to work out.

As to what Professor Ayrton said in regard to the armature,—as I think I explained, the whole reason for the projection on the base of the instrument was that it might be brought up close to an armature. There would be difficulty in introducing it in the Edison machine (which was referred to by Professor Ayrton); but we make a different type of machine where we can get all round the armature coil without difficulty. In regard to the coils used in my machines, we can get quite close to the cores, and do put the cores in position before using the machine. If we altered our type, I should adopt Professor Ayrton's suggestion, and alter the instrument accordingly; but, as it has been made for our own use, we find we can get all round the cores of our armature-coils with the way in which it is arranged, and can get at every part of the machine. I want to measure the field where the wire is, and where the current is generated, and there the instrument will go. I am quite aware that we do not get an accurate measurement, because the field is different when the machine is in motion to when it is at rest; but I do contend that with such an instrument we get a useful guide in designing the machine, and are more likely to approximate to a good machine after we have tried the magnets with an arrangement of this sort (imperfect though it may be) than if we were to build a machine without any experiment at all.

I was very much interested in what Professor Hughes said about the method of working with little bits of iron and small magnets——

Professor **Hughes**: Large magnets.

Mr. J. E. H. **Gordon**: Large magnets and small pieces of iron. The magnets that we use would pull a spanner out of a man's hand 6 or 8 inches away. I have seen a carpenter's wooden 2-foot rule placed against the pole of a magnet, standing straight out, caused by the magnet acting on the little clip put over the end of the rule to prevent fraying; and I am afraid that if we attempted to employ such magnets as Professor Hughes mentioned, they could not be held at any particular angle. The vibrating arrangement would be much more accurate for any small magnetic forces, such as the earth's force; but the method I have shown is meant as a practical one to get over the difficulty of using magnets when the field is strong enough to pull them out of their supports, or to demagnetise them.

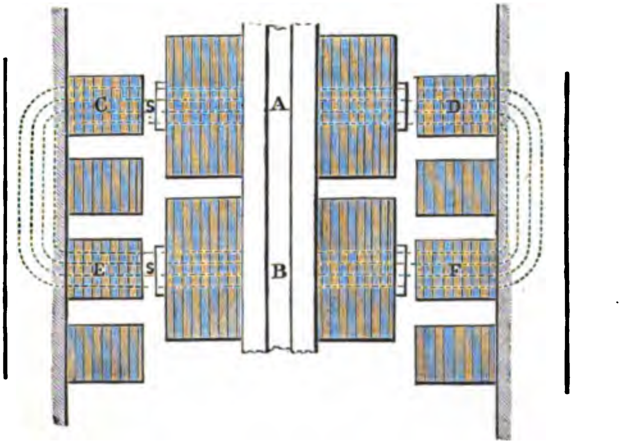
As to what Mr. Gisbert Kapp said about position, I should think that an alteration in position would make a difference. I have not made accurate experiments on that yet, but have been careful to put the instrument in the same relative position in each two cases. In fact, the practical way in which we use the instrument is rather more simple than that described.

Returning to the matter of the mains, I must point out to Mr. Crompton that I did not call my paper "a method of finally calculating the best proportion to give to mains," but "a method of calculating the horse-power expended in mains." I am quite aware that the horse-power expended in mains is not the only consideration which should guide us in settling our size; but, as I have said, the method of determining the fall of electromotive force is an extremely complex thing, but the horse-power expended is closely connected with it. When the horse-power expended is small, the fall of potential is small; and a calculation of the amount of horse-power wasted in the mains and the percentage horse-power so wasted, will give us a good deal of valuable information, and I do not claim more than that for the method; and having worked out this as a first approximation, any further information that we can, by calculation or experiment, get as to what the fall of potential may be, will be extremely valuable. There is nothing in the calculations that prevents us having feeders. I am not assuming that the calculations are final, but they give us a good deal of valuable information; and I think the practical way is to consider the network cut at certain points (and of course we put it together afterwards), and work out as near as we can what the modification ought to be. But Mr. Crompton's view, that it is necessary to make the conductor of the same section throughout, certainly startled me. I think the copper bill would be something very terrible; it seems to me that in that case there would be a great portion of the copper conveying much less current than it would reasonably carry, and in another portion too much current would pass. That is a question which only experience can decide, and which I hope both Mr. Crompton and ourselves will have an early opportunity of testing practically.

What I had to say as to Sir William Thomson's law has been anticipated by Mr. Blakesley. That law is very interesting scientifically, and would be of practical value if the conductor could be floated in the air; but it does not take into account the cost of insulation, or of boilers, dynamos, etc., all of which must be considered in making up the total expense; therefore I cannot see that the law, however interesting scientifically, is of any practical use.

Then Mr. Carus Wilson asked a question as to the number of lamps. We plot out on the map the number of lamps that each house is likely to require, and so ascertain, as nearly as possible, how many each main will have to supply, and a margin is allowed in excess of the probable number: that is as near as we can get in public district lighting. In private district lighting, where property is all under one hand, the number of lamps is known with accuracy, but with public lighting a guess is the only thing possible.

Professor **Ayrton** remarked that he had instanced an Edison or a Siemens machine, but that his observations would apply equally well to Mr. Gordon's own machine, shown in the following figure, where A and B were the field magnets, and C, D, E and F the armatures, or *vice versâ.* In either case the lines of force were something like the dotted lines shown in the figure, and the place where it was of practical importance to measure the strength of the magnetic field was between the faces of A and C, of B and E, that is, in the small space S, which, in order to make the machine



owerful was necessarily kept as small as possible. To measure he field in the space occupied by the wire on the armature, in the ompleted machine, before the armature was wound, which was all hat could be effected by Mr. Gordon's apparatus, was comparaively useless, the power of the machine depending, as it did, on he number of lines of force passing across the space S.

The **President**: Gentlemen,—We have had two very interestng papers, and, I think you will all admit, a very interesting and nstructive discussion. I am not surprised to find that there is a reat difference of opinion on the subject. Although, as I said efore, I am not immediately connected with electric lighting, I requently use my spare time in experimenting in this direction for my own information; and perhaps one day I shall give the Society the benefit of my researches. To-night is the first time I have seen the instrument Mr. Gordon has shown us, and I shall be glad if he will allow me to experiment with it; for the results I have obtained show that there is much to be learnt with regard to instruments for magnetic measurement. But however we may differ in opinions, I am sure you will be all in accord in acknowledging your thanks to Mr. Gordon for the two papers he has brought before us, and for the discussion they have evolved.

Professor W. E. **Ayrton**: As I have been rather critical, I shall have very great pleasure in seconding the vote of thanks to Mr. Gordon.

The President formally put the vote to the meeting, and it was cordially expressed.

A ballot took place, at which Mr. William B. Esson was elected as an Associate, and the meeting adjourned until the 10th January, 1884.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY TO NOV. 13, 1883.

By Alfred J. Frost, *Librarian.*

(*Works marked with an Asterisk (*) have been purchased.*)

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Board of Trade (Standards Department),
7, Old Palace Yard, S.W.,
5th September, 1883.

Sir,

With reference to previous communications on the question of a standard wire-gauge, I am directed by the Board of Trade to acquaint you that a new standard wire-gauge of the several sizes herewith enclosed has been now made and duly verified under the Weights and Measures Act, 1878.

I am also to enclose copy of a circular letter which has been addressed on this subject to the several local authorities under the Act.

I am, Sir,
Your obedient Servant,
Henry G. Calcraft.

F. H. Webb, Esq.,
Secretary,
Society of Telegraph-Engineers,
4, Broad Sanctuary, S.W.

"Board of Trade (Standards Department),
"7, Old Palace Yard, S.W.,
"*4th September*, 1883.

"The Weights and Measures Act, 1878.

"Sir,

"After consulting with the various institutions, local authorities, manufacturers, and others practically interested in the use of wire-gauges, the Board of Trade have caused a standard measure for wire, etc., to be made and duly verified, which measure has now been approved by Her Majesty in Council, to be, on and after the 1st day of March next, a Board of Trade standard under the Act.

"I am directed therefore by this Board to transmit to you a copy of the sizes of this new standard measure, and to ask for the co-operation of your local authority in giving publicity to it in their district, so that by the common adoption in manufacture and trade of a uniform gauge, the annoyance and loss occasioned by the present variety of sizes in use, of which complaint has been made, may no longer arise.

"I would point out that by the above Act it is provided that all measures in use in trade are to agree with the Board of Trade standards, and that it is the duty of the Inspector of Weights and Measures to see that the Act in this respect is carried out.

"I am, Sir,
"Your obedient Servant,
"Henry G. Calcraft."

NEW STANDARD GAUGE FOR WIRE.

LIST OF SIZES.

Descriptive Number, B.W.G.	Equivalents in parts of an Inch.	Metric Equivalents in Millimètres.	Descriptive Number, B.W.G.	Equivalents in parts of an Inch.	Metric Equivalents in Millimètres.
No.	*in.*	*mm.*	*No.*	*in.*	*mm.*
7/0	0·500	12·700	23	0·024	0·610
6/0	464	11·785	24	22	0·559
5/0	432	10·973	25	20	0·508
4/0	400	10·160	26	18	0·457
3/0	372	9·449	27	0·0164	0·4116
2/0	348	8·839	28	148	0·3759
0	324	8·229	29	136	0·3454
1	300	7·620	30	124	0·3150
2	276	7·010	31	116	0·2946
3	252	6·401	32	108	0·2743
4	232	5·893	33	100	0·2540
5	212	5·385	34	92	0·2337
6	192	4·877	35	84	0·2134
7	176	4·470	36	76	0·1930
8	160	4·064	37	68	0·1727
9	144	3·658	38	60	0·1524
10	128	3·251	39	52	0·1321
11	116	2·946	40	48	0·1219
12	104	2·642	41	44	0·1118
13	0·092	2·337	42	40	0·1016
14	80	2·032	43	36	0·0914
15	72	1·829	44	32	0·0813
16	64	1·626	45	28	0·0711
17	56	1·422	46	24	0·0610
18	48	1·219	47	20	0·0508
19	40	1·016	48	16	0·0406
20	36	0·914	49	12	0·0305
21	32	0·813	50	0·0010	0·0254
22	28	0·711			

CATALOGUE OF ENGLISH WORKS RELATING TO EARTH CURRENTS.

Compiled by Alfred J. Frost, Librarian.

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Discussion by—
Prof. W. G. Adams, F.R.S.
Mr. Whipple, Kew Observatory.
„ C. Wollaston.
Prof. J. Perry.
Mr. C. F. Varley.
„ W. Ellis.
„ S. M. Banker.
„ Willoughby Smith.
„ E. Graves.
„ C. E. Spagnoletti.
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Prof. W. E. Ayrton.

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Whipple [G. M.]
Perry [S. J.]
Capron [J. R.] and others
} The Aurora and Electric Storm of Jan. 31. [*Nature*, Feb. 10, 1881, pp. 348-351.]

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1881. **Spagnoletti** [C. E.] Effects of Flow of Water against or under an Insulated Wire. [*Journ. Soc. Tel. Eng.*, Vol. X., p. 277.] [See also *Electrician*, Aug. 27, 1881, p. 228.]

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1880. **Johkjennerud.** Earth Currents. [*Electrician*, Sept. 4, 1880, p. 190.]

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ORIGINAL COMMUNICATION.

HEMP- *VERSUS* IRON-COVERED CABLES.

By Jas. Graves.

Considering the importance of this question as affecting the future of submarine telegraphy, the discussion which followed the reading of Messrs. Trott and Hamilton's paper at the last meeting of the Society appears to have been of far too limited a character, and it might with possible advantage be renewed at a future meeting.

The speakers who carried on the discussion were gentlemen who have attained eminence in their profession, and they were no doubt able to speak with authority upon the phases of the question with which they were familiar; but their remarks (judging from published abstracts) were more in reference to what had been done many years ago than to the points brought prominently forward by the joint authors of the paper read.

Messrs. Trott and Hamilton, fellow-members whom I have not the pleasure of knowing personally, have been exclusively engaged in repairing cables, both old and comparatively new, for several successive years, and have obtained for themselves a name and a credit for expedition in their work unsurpassed by any electrical engineers who have ever been engaged in similar work.

Captain James Blacklock of the "Old Monarch" was a very successful navigator in connection with cable repairs, and with whom, as submarine electrician to the Electric and International Telegraph Company, I co-operated for several years in the North Sea, and again, in 1870, in repairing the Anglo-American Telegraph Company's 1866 cable off Newfoundland, in the "Robert Lowe," and then proceeded to repair the French Atlantic Company's cable off Nova Scotia; and whether in the German

Ocean, between the Channel Islands, or in the North Atlantic, I could but admire the tact, skill, and accuracy displayed by him in his portion of the work. In mentioning this fact, I am only doing justice to the memory of one whose name appears to be almost, if not quite forgotten.

It is this kind of work—the picking up of *old* cables and the repairing of them when their protecting sheathing is partly or entirely worn out—that gives practical men an advantage over those who deal chiefly with theoretical principles or with *new* work; and this should give great weight to the testimony of their repeated experiences, and observation of numerous undeniable, although perhaps exceptional phenomena which have passed before their eyes in a series of years exclusively devoted to such work.

Theorists may argue that this or that is an impossibility; but if practical men say with all seriousness they have seen the thing done, there should be an end for ever to the imaginary impossibility; and when such men as Captain Trott and Mr. Hamilton (men who have been notoriously successful in their work) speak of what they have seen occur repeatedly, there should be a disposition on the part of those who have not seen to believe, and not to doubt what they say. Men like them, engaged exclusively on work of this kind, have plenty of time and opportunity to compare recovered ends of broken cables, to reason from the evidence the causes for breakage in the first place, and also of those which have occurred during the operation of repairing, whether in picking up or paying out; and they are far more likely to come to a just conclusion as to *causes* than electrical engineers who have on but a few occasions been engaged in such work, or others who perchance have never seen a cable repaired.

The causes of breakage having been traced, the next step is to consider the remedy. Messrs. Trott and Hamilton suggest the abolition of iron and the substitution of another material, if one can be found suitable, or, in the absence of that one, a covering entirely of hemp of a specially durable nature, similar to the rigging of the "Royal George," or the Russian hemp exhibited at the meeting, to be laid on in a particular way to prevent kinking.

It has been suggested that a hemp-covered cable might not prove so suitable as an iron-covered one for bridging over chasms at the bottom of the ocean. Assuming that the cable is hanging suspended from two points, the question required to be demonstrated is, whether the extra weight of an iron-covered cable would not cause more abrasion at the points of suspension than a hemp-covered one, which would be proportionately lighter and more buoyant, and the result prove that the iron would be worn through quicker than the hemp.

The oxidation of the iron in salt water has the effect not only of weakening itself, but also of deteriorating the strength of the hemp, and thus becomes doubly objectionable.

Another comparative phase of the question is, that in the hemp-covered cable there would be an entire absence of that suicidal element, the piercing of the core by a sharp point of iron, two of which would be produced as soon as one iron wire was worn through, two more for the next, and so on, until perhaps one-third of the wires were worn through; and then the weight of the suspended cable would tend to give this weak spot a wrench or twist, probably turning the cable half round, and thereby bring not only fresh wires into the area of friction, but also the sharply pointed ends (some of which may have got bent in the movement) opposite the core, and the weight of the cable forcing the core upon these points, until they, or one of them, reach the conductor, when of course the life of the cable ceases, and then an iron-covered cable is said to have failed (or parted) in deep water from some "mysterious agency." Such a case as this would bear out the observed facts of cables twisting at their weak parts when a strain is brought to bear on them in lifting, or by being suddenly strained after being checked whilst being paid out, with kinks formed by undue slack at the bottom.

Iron sheathing is no doubt necessary, or some other durable substitute, to resist the rough usage to which shore ends of cables are exposed from abrasion on rocky ground, and between or beneath boulders on the beach; but even heavy shore ends have more than this to contend with when they cross unseen and unknown metallic beds, which consume them by slow but sure degrees.

Perhaps it will scarcely be credited that a shore end, weighing 19 to 20 tons per knot, on the west coast of Ireland, covered with solid iron rods half an inch in diameter, was, some nine years ago, eaten up into a series of short lengths varying from less than 2 feet up to a few fathoms, several being less than 2 fathoms, and the cable being held together by the bare core. I have before me a piece of this iron, 19 inches long, the middle 10 inches of which is half an inch diameter, while the two ends are tapered to sharp points. Several pieces were recovered where every one of the iron rods was eaten through, and their ends tapered to points as sharp as bradawls.

In picking up this cable—eaten up by galvanic action on metallic ridges at short distances apart—the bare core had to bear the whole weight in lifting, and in one case two such places were included between the bow-sheave and the water. A hempen hawser could not possibly have met with such a fate.

On this same shore end a double serving of hemp was laid right-handed and left-handed, and over all a covering of preservative compound, forming a massive cable 8 inches in circumference. The lay of the hempen yarns was very short. A piece of this external covering now before me shows, that while the top and sides of it are covered with a species of silica or coral, the bottom is worn quite through longitudinally, and the natural consequence of such wear and tear is simply to reduce the yarns laid on in contrary directions to lengths of about 4 or 5 inches each, and of course all the lifting strength is gone; and the presence of such a mass of material, enclosing the cable for three-fourths of its circumference, would prove mere dead weight when lifting it, as this was lifted, from a depth of 40 fathoms.

And in this particular I cannot quite agree with Messrs. Trott and Hamilton in placing implicit reliance upon hemp for lifting-strength when laid up spirally around the core; although several layers may be superposed, each contrary to the other in direction, and the lay of the yarns in each case contrary to both, as they suggest, to avoid kinking or buckling up when submerged.

It will be seen that, if from any cause the hemp be worn through on one side to the core, the whole lifting-strength of the covering would be gone, as all the yarns would be separated as soon as the length of one revolution of the lay had been worn through. This naturally suggests that the lifting-strength in a hemp cable should be longitudinal with the core, around which the usual jute padding should be put to increase its diameter, as well as to form a soft bed for the core. Over the jute padding should be laid longitudinally as many of the strongest hempen cords as would pack closely round it, and then over these should come the servings suggested by Messrs. Trott and Hamilton. Should the outer spiral coverings get worn through, or even in addition two or three of the cords, there would still remain three-fourths of the strength, in addition to the core, for lifting purposes, instead of the whole strength being lost and the core itself saddled with the whole weight.

There are inconveniencies attending iron-covered cables which develop themselves during the practical work of repairing them, especially when the outer wires are large, such, for example, as the Dunwich-Zandvoort, or the Lowestoft-Zandvoort cables, in which I have seen faults caused by broken sheathing-wires penetrating the cores. In picking up or under-running such cables, wires constantly snap at bad welds or brittle places, frequently a few inches on either side of the weld itself, when on the grapnel or passing over the bow-sheave, and get foul of the gear, which rips the wire for several feet out of its place on the cable. It was to avoid this ripping effect by ships' anchors, when getting foul of a broken wire on catching the cable, that the late Mr. C. F. Varley decided on using stranded shore ends, suggested, I believe, by Mr. W. H. Preece, to limit this action to a few inches only.

Nothing of this kind could occur with a hempen cable; and I think the operation of splicing a hempen cable at sea would be far easier and more quickly done than the same operation with an iron cable, and there would certainly be the satisfaction of knowing that no iron ends, whether blunt or pointed, butted together or not, and no iron lapping wires, were left behind to stand their chance of producing a fault at some distant date.

The operation of picking up and repairing hempen cables would be a much cleaner one than similar work with iron cables with their oxidation falling off in scales, black mud, and submarine deposits all discoloured, to say nothing of the rough nature of the work in handling half-corroded iron with sharp points and rough places here and there, often tearing the cablemen's hands, and the operation of stripping old short pieces in order to save the core and throwing overboard the useless iron.

All things considered, if longitudinal strength can be embedded in the cable near the core, I think there is much to be said in favour of hempen cables in preference to iron-covered ones for deep water.

Extra coverings of galvanised iron could be used for the shore ends, to resist abrasion from rocks and stones in the tidal wash till deeper water is reached, as these portions could be more easily replaced. The iron of the shore ends thus covered would serve as now for the working earths; otherwise, in the event of the shore ends being entirely of hemp, special earth-cables would have to be laid for working purposes.

VALENTIA, *December* 14, 1883.

ABSTRACTS.

LUCIEN J. BLAKE—EVAPORATION A SOURCE OF ELECTRICITY; AND NEUTRALITY OF VAPOUR FROM ELECTRIFIED FLUID SURFACES AT REST.

(Annalen der Physik und Chemie, B. XIX., H. 3, No. 7, 1883, pp. 518-534.)

In the first part of his paper, the author gives a description of the experiments he has made on the production of electricity by evaporation.

The movable plate, A, of a condenser, made in accordance with Kohlrausch's pattern, was in connection with a Helmholtz quadrant electrometer, the quadrants of which were charged by two of Zamboni's dry piles. The aluminium needle, in the form of two sectors joined by their apices, was suspended by a wire, which also served as conductor, to a torsion head; the metal body of the instrument was put to earth. A Daniell's cell, with one pole to earth, gave a deflection of 67 to 71½ mm. on a scale distant about 3 mètres from the mirror.

The brass rod of the fixed plate, B, of the condenser was prolonged, and its other end formed into a ring. In this was placed a porcelain cup filled with sand, in which was a second small porcelain cup. The important point was the insulation of the support of the plate B, and this was carefully measured before and after each experiment. When B was charged by four Daniell's cells, giving a deflection of 140 divisions, the loss from leakage was only about 4½ to 7 per cent. after ten minutes. It was observed that if left to itself the plate B became slightly charged; but this charge was always measured and allowed for.

The liquid having been placed in the inner porcelain cup, the sand-bath was heated up to 100° C., and then the flame extinguished. A platinum wire, fused into a small glass tube, dipped at one end into the liquid, while the other end was connected to the plate B of the condenser. The condenser plate A, which was ½ mm. from B, as well as the aluminium needle of the electrometer, was put to earth. After 10 or 15 minutes, A was insulated and removed from B. The deflection of the needle was then taken as a measure of the electrical condition of A, and therefore of B.

The mean values deduced from the experiments are tabulated on next page, the deflections being given in divisions of the scale, 45 of which corresponded to one Daniell's cell.

	At beginning.	After 5 mins.	After 10 mins.	After 15 min.
. Sea-water at temperature of room	0	− 0·71	− 1·02	− 1·40
2. Sea-water at 100° C.	0	− 0·16	− 0·37	− 0·66
3. Sulphate of copper at temperature of room	0	− 1·2	− 1·2	...
4. Sulphate of copper at 100° C.	0	− 0·94	− 2·3	...
5. Chloride of sodium at temperature of room	0	− 1·0	− 1·5	...
6. Chloride of sodium at 100° C.	0	− 1·5	− 1·5	...

A second method of observation was tried, in which a circular brass disc 125 mm. in diameter was fastened in a horizontal position to a wire which was connected without a condenser directly to the aluminium needle of the electrometer. Immediately under the disc C was the cup containing the liquid, which was connected to earth. On evaporating the liquid by a sand-bath, the vapour was condensed on C. The zero point having been fixed, the plate C was insulated, and the deflection read, after which the zero was again determined. In these experiments 70 divisions corresponded to one Daniell's cell. The results obtained were—

Sea-water at Temperature of Room.

	Zero at commencement.	Deflection after 5 min.	Zero after experiment.	Change of Zero.	Charge after 5 min.
First experiment ...	690	689·0	690	0·0	+ 1·0
Second „ ...	571	569·5	570	+ 1·0	+ 0·5
Sea-water at 100° *C.*					
First experiment ...	689	691	690	− 1·0	− 1·0
Second „ ...	568	567	567	+ 0·5	+ 0·5

The second portion of the paper deals with the hypothesis that a convection of electricity occurs through vapour arising from the surface of an electrified liquid.

A film of water, which had been condensed on the movable plate B of the condenser, the plate A of which was connected to the needle of the electrometer, was warmed while in connection with one pole of a battery, the other being to earth, and the steam recondensed on A. The battery power was increased from 1 to 404 Daniell's cells, but no transference of electricity was observed, though from the care with which the experiments were conducted any charge on A would have been at once apparent. Further experiments were made, with a somewhat modified apparatus, with various liquids, as solution of common salt, alcohol, sulphuric acid, and mercury. In some cases a battery equivalent to 404 Daniell's cells, and in others a Tobler's frictional machine, was used to charge the liquid, but the final result of all the researches was opposed to the generally-received theory, and proved, according to the author, that the vapour arising from the surface of a liquid at rest is electrically neutral.

J. ELSTER and H. GEITEL—PRODUCTION OF ELECTRICITY BY CONTACT OF GLOWING BODIES AND HOT GASES.

(*Annalen der Physik und Chemie, B XIX., H. 4, No. 8a, 1883, pp. 588-624.*)

The experiments described in this paper were undertaken by the authors in order to extend their theory of the electricity of flames.

Their former experiments, which have been described in the *Annalen* (No. 16, 1882, pp. 193-222), were carried out with a wire rendered incandescent by the passage of a current through it. In the present case they preferred to use a so-called *Paquelin* burner. A sphere of stout platinum foil is attached to one end of a hollow brass tube, the other end of which is in connection with a bottle containing benzine. A stream of air is forced through the benzine under a regulated pressure, which is measured by a manometer, and the vapour burns inside the brass tube and the platinum sphere, a side opening being provided for the escape of the products of combustion. It was found that the temperature of the platinum sphere depended on the pressure under which the stream of air was forced through the benzine. In order to have a standard of comparison, a wire of platinum 3 mm. thick was fixed about half a mètre from the sphere, and was maintained at a constant temperature by a Bunsen flame. This wire was at the same height as the sphere, so that it might be seen projected on the surface of the latter. If any difference in the glowing of the wire and sphere was observed, the latter was readily brought to the constant temperature by regulating the pressure of the air stream by means of a tap.

The burner was fixed horizontally in one side of a zinc box, so that the platinum sphere was at about its centre. Above the sphere was a platinum crucible filled with distilled water, and closed by a lid of brass from which a tube also of brass projected up through the top of the zinc box, being insulated from it by passing through a glass tube. By means of two openings the zinc box could be filled with the gas under observation. The glowing platinum sphere was connected to earth, as were one pair of quadrants of the electrometer. The other pair of quadrants were connected to the so-called "air-electrode," or platinum crucible; while the electrometer needle was kept constantly charged by Zamboni dry piles. A preliminary series of experiments with dry air showed that for small differences the E.M.F. was proportional to the pressure in the manometer.

The next gas experimented with was carbonic acid as compared with dry air. One Daniell's cell gave a deflection of 53 divisions of the scale; with dry air 77·1 were noted; with the carbonic acid gas evolved by the action of hydrochloric acid on marble and thoroughly dried, 77·9, and with air again, 77·4. If we put $E_D = 100$, we have $E_A = 144·5$; $E_{CO_2} = 146·9$; $E_A = 146$; or as mean values $E_A = 145·7$; $E_{CO_2} = 146·9$; from which we may assume that the action of carbonic acid gas is about the same as that of air.

Ordinary coal gas was then tried, and gave $E = 96·6$; while air in this case gave $E = 97·7$, so that, again, no decided difference was observed.

With oxygen no difference was remarked, as may be seen from the results of three series of experiments. As comparison one Daniell equals 100 divisions:—

	E_0	E_A	$E_0 - E_A$
Series 1.	88·2	87·5	+0·7
„ 2.	131·4	133·0	−1·6
„ 3.	157·1	157·2	−0·1

These small differences were most likely due to errors of observation.

A very careful set of experiments on the influence of water vapour in air gave the following results:—One Daniell = 100—

Mean for dry air	160·6
Air of room	158·0
Saturated air	159·0

The authors also made use of another form of apparatus, in which a thin tube of platinum or brass was heated near one end by a Bunsen burner. The tube, as well as the burner, was connected to earth, while a stream of air was forced through the tube and impinged on a metal plate serving as electrode of the electrometer. The maximum effect was obtained if the heated point was so near the end of the tube as to cause the latter to glow. The charge on the plate increased rapidly to a maximum of about 5·5 to 6 Daniell's, at which it remained constant; the charge on this plate was always positive. The rapidity with which the plate attains its maximum charge is proportional to the number of particles of gas which impinge on it in unit of time.

Experiments were also tried with hot gases and cold metallic bodies, as well as with glowing non-metallic bodies. The results obtained lead to the following laws:—The particles of the gases experimented with, viz., air, coal gas, carbonic acid, and oxygen, become positively electrified on contact with hot bodies, whilst the hot body shows negative electricity. The charge of a cold body introduced into a stream of hot air is greater the more brightly the body producing the electricity glows. It is independent of the nature of the gas and of the electrode.

The paper then deals at considerable length with the electricity of flames, and the authors sum up the results of their experiments as follow:—

1. There is no electrical difference between the base and point of a flame. A difference is only caused if glowing solid bodies are introduced into the flame.

2. The apparent polarisation of the flame in cross section considerably exceeds the polarisation in the direction of its length; since if one electrode is

entirely removed from the flame, it is only touched by heated molecules of gas which do not glow.

3. The volume of the gas into which the two electrodes are plunged is indifferent. Hence for the same electrodes the electricity produced must be the same for a large flame as for a small one.

4. The change of polarity of the flame for small changes of position of the electrodes is limited by the fact that the electricity always spreads itself freely above the flame.

5. Since the introduction into the flame of volatile salts increases the number of glowing particles, the E.M.F. must also be increased.

6. The E.M.F. depends on the nature of the metal of the burner.

7. The nature of the electrodes has no effect.

8. A white-glowing platinum wire in the flame must be charged just as in air. The electrical difference between the two must be *nil*, since all gases become equally positively charged.

9. For the same reason, a transformation of the process of combustion can have no effect on the polarity of the electrodes.

10. Flames which are in contact with no metal, or with cold ones, show free negative or positive electricity accordingly as the connection of the one or the other to the earth is better. The E.M.F. in this case is caused by the contact of hot particles of gas or air with the glowing particles of solid bodies suspended in the flame, or with the glowing molecules of the gas itself.

11. Since free electricity is produced in every flame, two flames affect each other electrically, if not too far apart.

12. Since the particles of gas leave a flame electrified, the air of a room in which a flame is burning is feebly charged with positive electricity.

Hence every flame may be considered as a stream of hot gas, which charges with negative electricity glowing electrodes plunged into it, as well as the glowing particles suspended in it.

W. HANKEL—ELECTRICAL PROPERTIES OF QUARTZ.

(*Annalen der Physik und Chemie*, B. XIX., H. 5, No. 8b, 1883, pp. 818-844.)

The conclusions at which the author arrived in two former articles on the same subject having being questioned by C. Friedel and J. Curie, he restates his deductions, and brings forward the evidence of further experiments in support of them.

I. Thermo-Electricity.

1. Rock crystal, the purest kind of quartz, generally occurs in hemimorphic crystals, formed by the development of the trigonal trapezohedra and trigonal pyramids.

2. Opposite electrical poles are produced by changes of temperature at both ends of each of the hemimorphic minor axes, so that positive poles and negative poles alternate around simple crystals.

3. With a rise of temperature the polarity is the opposite to what it is with a fall.

4. With a falling temperature the positive poles are situated at those axes at which the faces of the trigonal pyramids and trapezohedra appear. With a rise of temperature the opposite takes place; or, in other words, on heating, the thermo-electric positive poles are produced on those edges which do not bear any trigonal forms, the negative poles on edges which have such forms. The opposite action has been stated to take place by C. Friedel.

In his former experiments the author placed the crystal on a metal plate, and determined the electrical tension by touching the surface with the point of a very fine insulated wire in connection with a gold-leaf electrometer. In his later experiments he buried the crystal up to the face on which he wished to experiment in a mass of copper filings, so as to get rid of the opposite polarities developed on the other faces. The copper filings were contained in a copper box, which could be heated in a furnace. The temperature of the crystal was determined by placing on the edge a small quantity of stearine, which melted at 51·8° C.; this served as control to a thermometer which was plunged into the copper filings. It was found that when the stearine melted the thermometer marked 52° C., while at 51° C. on the thermometer the stearine was still solid. Hence it was evident that the copper filings and the rock crystal had very nearly the same temperature. The experiments showed that, whether the free surface of the crystal was warmer or cooler than the buried part, it made no difference to the conclusion arrived at by the author. Contrary to the results obtained by Friedel and Curie, the author found that a crystal which was allowed to cool freely in air showed always traces of electricity.

5. In normal simple crystals there is a difference in the spreading of the two electrical polarities over the surface, accordingly as the crystal is right or left handed; the distribution depending apparently on the position occupied by the pyramidal surfaces.

6. In twin crystals each shows a polarity corresponding to what it would have if single.

7. The ends of the principal axis always show the same polarity; in the case of cooling, both ends are positive.

8. If heated above 200°, the crystal loses all traces of electricity.

II. ACTINO-ELECTRICITY.

1. If heat rays fall on a simple rock crystal, electric polarity is produced at the ends of the secondary axes and on the six edges of the prism.

2. The difference of potential increases at first rapidly, and afterwards more slowly, reaching its maximum in about 40 minutes.

3. The difference of potential disappears in the same time when the rays are cut off; at first quickly, then more slowly.

4. Positive poles are produced on the three edges on which the surfaces of the trigonal forms would be if they were developed, negative poles on the other three.

5. The direction of the heat rays is indifferent; they may even pass through the crystal parallel to the principal axis.

6. The source of heat is indifferent—flames, Leslie's cubes, heated metal balls, etc.

7. The difference of potential is proportional to the intensity of the radiation.

8. The rays which give rise to actino-electricity are not quite the same as those which affect the blackened end of a thermopile.

9. Twin crystals show individual polarity.

10. These phenomena are reversible, and can be produced by radiation *from* the quartz crystal.

MM. Friedel and Curie have not observed these phenomena, and think that they are not merely produced by unequal heating of the crystal. The author, however, cites his former observations, as well as many new ones, in support of his results as stated above.

III. Piezo-Electricity.

1. By exerting a pressure in the direction of one of the minor axes, the edges which have trigonal forms show negative electricity, and the others positive. When the pressure is taken off the opposite polarities are produced.

2. Pressure in the direction of one minor axis produces polarity at the ends of the other two corresponding to an expansion.

H. MEYER—DEPENDENCE OF THE MAGNETISATION OF STEEL ON ITS HARDNESS.

(*Annalen der Physik und Chemie*, *B.* *XIX.*, *H.* 5, *No.* 8*b*, 1883, *pp.* 849-856.)

From some previous experiments the author had been led to the conclusion that the coefficient of magnetisation for all kinds of steel does not diminish when the degree of hardness is increased, and he undertook further experiments in order to verify this conclusion.

Five bars in their original hard state were first tested for conductivity, and for their coefficient of magnetisation for the inductive action of the vertical component (4·006) of the earth's magnetism, and were subsequently heated for several hours in a fire, and left to cool gradually in the ashes. The specific gravity, conductivity, and coefficient of magnetisation were then again determined.

The results may be summed up thus:—By hardening, the volume of the rods was diminished, while the specific gravity was greater after hardening than before; the coefficient of magnetisation and the resistance were both increased. In some cases the resistance was determined before the coefficient of magnetisation, and it was found that the passage of a current of electricity through the rod had little or no effect on the latter. The statement that the coefficient of magnetisation in some kinds of steel increases with the hardness, is opposed to Lamont's law—that the harder the steel is, the lower is this coefficient.

Following Weber, the coefficient of magnetisation is proportional to the number of molecules in unit of volume, and to their magnetic moments, and inversely to the molecular directive force; or, in symbols,

$$\gamma = \tfrac{2}{3}\,\frac{n\,\mu}{D}.$$

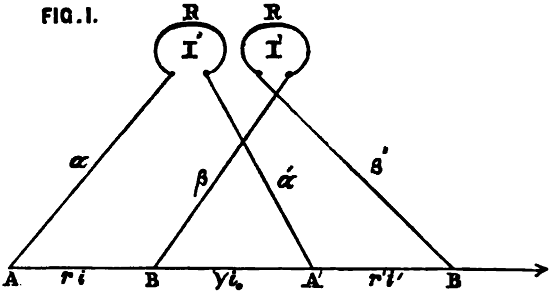
If now we suppose that by hardening, only the relative position of the molecules is altered, but that the magnetic moment of the individual molecules is the same both before and after, the increase of the coefficient of magnetisation can only arise from an increase of the number of molecules in the unit of volume, or from a decrease in the molecular directive force, or from both these causes simultaneously. But the number of molecules in unit volume is proportional to the specific gravity, and this does not change proportionally to the hardness; so that only the directive force, D, can have an effect on the coefficient of magnetisation.

F. KOHLRAUSCH—COMPARISON OF ELECTRICAL RESISTANCES INDEPENDENTLY OF THE RESISTANCE OF THE LEADS.

(*Annalen der Physik und Chemie, B. XX., H. 1, No. 9, 1883, pp. 76-87.*)

Sir Wm. Thomson's valuable modification of the Wheatstone bridge for small resistances is only very slightly affected by the resistance of the leading wires. The differential galvanometer may be used for the same purpose as has been shown by Kirchhoff, if the coils are connected in series and opposed to each other.

FIG. 1.



The two resistances to be compared, r and r', are joined up between A B and A′ B′ respectively in the same circuit. The two coils of a differential galvanometer are connected up as shunts to the two resistances, so that the current passes through them in opposite directions, and the connection B A′ is included in both, as shown in the diagram (Fig. 1); the two ends A and A_1 of each wire are connected to the one coil, and the two ends B and B_1 of each wire are connected to the other coil.

If the several resistances and currents are represented by the letters affixed to them in the diagram, then if the needle points to zero, and $R = R'$ (*i.e.*, the two coils of the differential galvanometer have equal resistance), it is clear that $I = I'$ and $i = i'$; also

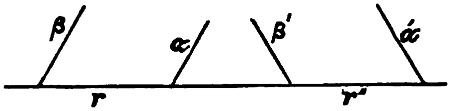
$$ri + \gamma i_0 = (R + \alpha + \alpha')\,I$$
$$r'i + \gamma i_0 = (R + \beta + \beta')\,I$$

where $$i_0 = i - I.$$

Hence $$\frac{r + \gamma}{r' + \gamma} = \frac{R + \gamma + \alpha + \alpha'}{R + \gamma + \beta + \beta'}.$$

Secondly, the connections are interchanged, without altering the leads to the galvanometer, so that the points, which before were innermost, are now outside, and *vice versa*, thus:

FIG. 2.



It will then be necessary to change r' to r'' to bring the needle again to zero.

Then we have again

$$\frac{r + \gamma}{r'' + \gamma} = \frac{R + \gamma + \beta + \beta'}{R + \gamma + \alpha + \alpha'}.$$

From these two equations we get

$$r + \gamma = \sqrt{(r' + \gamma\,(r'' + \gamma)}$$

If now r' and r'' differ very slightly we may take, instead of the geometrical, the arithmetical mean value,

$$r = \tfrac{1}{2}\,(r' + r''),$$

and we shall thus introduce an error,

$$\tfrac{1}{2}\left(\frac{r' - r''}{r' + r''}\right)^2 = \tfrac{1}{8}\left(\frac{r' - r''}{r}\right)^2$$

which is without appreciable effect, since $r' - r''$ is very small as compared with r. Hence we obtain the rule to make two determinations with reversed connections, and then the resistance sought is equal to the arithmetical mean of the two comparative resistances.

The author considers the means which should be adopted so that the two coils of the differential galvanometer may have an equal influence on the needle, which may be brought about by very careful winding; and a method for eliminating any difference of action which cannot be got rid of in the construction.

He also gives a convenient arrangement, shown in Fig. 3, by means of which the connections may readily be interchanged, these being made by mercury cups, indicated by the six small circles.

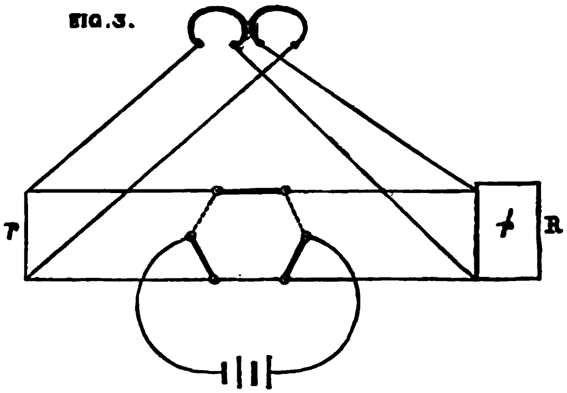
The connection pieces shown in full lines are used in the one measurement, the dotted ones in the other. R is a resistance box, by means of which the small change from r' to r'' can be effected.

The following experiment may serve as an example of the delicacy of the method:—Three similar pieces of German silver were taken, each 1·4 mm. in diameter, and 0·19 m. long, having approximately a resistance of one-hundredth of a Siemens mercury unit. Each wire was compared with each of the other two, and the results, expressed in *millionths* of a unit, are given below:—

I.–II.	=	+ 1·20	+ 1·26	+ 1·33
II.–III.	=	+10·98	+10·89	+10·86
III.–I.	=	−12·17	−12·30	−12·22
Sum	=	+ 0·01	− 0·15	− 0·03

Hence the hundred-thousandth part of the hundredth of a Siemens unit could be determined with ease.

FIG. 3.



F. KOHLRAUSCH—SOME METHODS OF DETERMINING THE ABSOLUTE RESISTANCE OF A CLOSED CIRCUIT, COMPRISING AN EARTH-INDUCTOR AND A GALVANOMETER.

(*Annalen der Physik und Chemie, B. XX., H. 1, No. 9, 1883, pp. 87-91.*)

This is an account of some improvements on the method used by the author at Gottingen for the determination of the Siemens mercury unit in absolute measure.

The original method consists in observing, besides the deflection caused by the induced current, the damping of the needle, and determining the constant of the galvanometer from this latter, and the time of oscillation and moment of inertia. Let

F = effective area of the coil of the inductor,
H = horizontal earth's force,
K = moment of inertia of needle,
t = time of oscillation,
λ = logarithmic decrement,
α = deflection of needle without damping,

$$\text{Then} \quad R = \frac{8\,F^2\,H^2\,\lambda\,t}{\pi^2\,\alpha^2\,K} \quad \ldots \quad \ldots \quad .. \quad \ldots \quad (1)$$

This method is, however, very difficult, but may be simplified. We may substitute another value for K. The needle is placed in an east and west direction in a bifilar suspension, and the deflection measured. Then $K = \frac{t^2}{\pi^2} \cdot M H$ where M is magnetic moment of the needle, and (1) will become

$$R = 8 \frac{F^2 H^2}{a^2} \cdot \frac{\lambda}{t} \cdot \frac{1}{M H} \quad \dots \quad \dots \quad \dots \quad (2)$$

or, dividing out by H,

$$R = 8 \frac{F^2 H \lambda}{a^2 M t} \quad \dots \quad \dots \quad \dots \quad \dots \quad (3)$$

in which equation the ratio $\frac{H}{M}$ is easily determined by the deflection method.

A further simplification may be introduced if, instead of calculating the constant of the galvanometer from the damping, we arrive at it by comparison with a known galvanometer, as has been done by Dorn. (See Abstract in No. 48 of Journal, p. 282.) It is the statical constant, c, which is thus determined, and not the dynamical, C. We have

$$C = M c = \frac{\pi^2 K}{t^2 H} \cdot c,$$

and from Weber's formula for the couple of a damped needle we have

$$\lambda = \frac{1}{2 R} \frac{t}{K} \cdot C^2 = \frac{\pi^4}{2} \cdot \frac{1}{R} \cdot \frac{K}{t^3} \cdot \frac{c^2}{H^2},$$

substituting in (1)

$$R = 2 \pi \frac{F}{a} \cdot \frac{c}{t} \cdot \quad \dots \quad \dots \quad \dots \quad (4)$$

in which expression all quantities are only raised to the first power, and K and H are eliminated.

Finally, from the above value for λ we get

$$R = \frac{\pi^2 c^2}{2 \lambda t} \cdot \frac{M}{H}.$$

J. PAKTOWSKY—EFFECT OF MAGNETISATION ON THE CONDUCTIVITY OF LIQUIDS.

(*Beiblätter, B. VII., St. 3, No. 3*, 1883, *p.* 202.)

A long tube, filled with a concentrated solution of chloride of iron, was placed inside a series of powerful solenoids. In order to minimise the polarisation, both ends of the tube were closed with bladder, and dipped in vessels full of solution of sulphate of zinc provided with amalgamated zinc electrodes. The tube formed one branch of a Wheatstone bridge, in which circulated a current from six Daniell cells. The author obtained a negative result. Whether the current was circulating or not in the magnetising coils, no difference in the resistance of the chloride of iron was observed. From some preliminary experiments the author concluded that an alteration of $\frac{1}{150000}$ would have been apparent.

C. E. GUILLAUME—ELECTROLYTIC CONDENSERS.

(*Beiblätter, B. VII., St. 5, No* 5, 1883, *p.* 406.)

The electrodes of platinum were placed opposite each other in a paraffin trough containing the liquid to be experimented on. The sides of the plates not opposed to each other were covered with a layer of insulating varnish. After this cell had been charged it was discharged through a mirror galvanometer, which was calibrated in absolute units. One hundred divisions of the scale corresponded with the discharge of a condenser of 275 microfarads when charged by one Daniell cell of E.M.F. $= 1 \cdot 106 \times 10^8$ cm.$^{\frac{3}{2}}$ g.$^{\frac{1}{2}}$ s.$^{-2}$ The greatest observed deflections corresponded with a capacity of 500 to 1,000 microfarads per square centimètre. If the cell was alternately charged with high and low potentials, the capacity increased. The maximum capacity occurred with 0·7 Daniell. On continued charging and discharging with the same potential, a maximum capacity was also reached with 0·7 to 0·8 Daniell. For a charge of one second, the ratio between the first residual discharge and the principal discharge was 0·01 to 0·07; for five seconds charge it was 0·09 to 0·13.

Stirring the liquid had no effect, and the distance of the electrodes a very slight one. The capacity increased about 3 per cent. for each degree C. of rise of temperature.

To test the effect of small differences in the time of charge, a pendulum was made to swing over strips of copper of various breadth, with which it made contact by means of a brush. With a 22 per cent. solution of sulphate of copper, and one Daniell for charging, the discharge curve is expressed by a parabola of the form $e^2 = 200\ t$, where t varies from one second to 0·03 of a second.

If a considerable time elapses between the charge and the discharge, a part of the charge disappears; the more, the shorter the duration of the charge. When the resistance of the electrolytic cell was measured by a Wheatstone bridge, it was found that the resistance altered with the charging potential.

With pure water, the capacity changes very little with charging potential; the duration of the charge has more effect. The discharge is very slow. The capacity is nearly inversely proportional to the distance of the electrodes. In weak solutions of sulphate of copper and of sulphuric acid the capacity increases with the degree of dilution. In general, in solutions of salts the capacity is smaller the greater the resistance.

According to one of the usually accepted hypotheses, the electricity in the molecules of a solid dielectric is first separated by the action of the external electric force; according to the other the already charged atoms determine the direction of the molecules. With liquids, the first hypothesis leads to the same result as with solids; according to the second the elastic forces have to be taken account of. Apparently the molecules arrange themselves readily through very small forces; for the action of the electrolyte on the electrodes must alter very little with the charging potential, therefore the capacity of the cell must be inversely proportional to the potential. Further, the action of a liquid must be equal to the sum of the actions of its molecules; apparently the capacity is proportional to the concentration.

The researches of the author entirely contradict these conclusions: he considers the phenomena as limited by the action of the surfaces of contact. If the electrodes have the potentials P_a and P_b, and are connected through resistances r'_a and r'_b with points of potential P''_a and P''_b; if the liquid surfaces in contact with them have resistances r_a and r_b and potentials P'_a and P'_b, and joined through a resistance r, then P_a P'_a, P_b P'_b are variable, the other quantities, including r_a and r_b, being taken as constant.

If C_a and C_b are the capacities of the surfaces of contact, we obtain four simultaneous differential equations—

$$\frac{P''_a - P_a}{r'_a} - \frac{P_a - P'_a}{r_a} = C_a \frac{d\,P_a}{d\,t};$$

$$\frac{P_a - P'_a}{r_a} - \frac{P'_a - P'_b}{r} = C_a \frac{d\,P'_a}{d\,t};$$

$$\frac{P'_a - P'_b}{r} - \frac{P'_b - P_b}{r_b} = C_b \frac{d\,P'_b}{d\,t};$$

$$\frac{P'_b - P_b}{r_b} - \frac{P_b - P''_b}{r'_b} = C_b \frac{d\,P_b}{d\,t}.$$

If we consider now a condenser of which the surfaces have potentials P_a and P_b, and are joined to points of potentials P'_a and P'_b through resistances r_a and r_b, whilst the internal resistance is r, we arrive at only two equations—

$$\frac{P'_a - P_a}{r_a} - \frac{P_a - P_b}{r} = C \frac{d\,P_a}{d\,t};$$

$$\frac{P_a - P_b}{r} - \frac{P_b - P'_b}{r_b} = C \frac{d\,P_b}{d\,t}.$$

Whence if r is very large in comparison with r_a and r_b, we have

$$P_a = P'_a \div C_1\,e^{-\frac{t}{c\,r_b}} + C_2\,e^{-\frac{t}{c\,r_a}}$$

Dr. R. ULBRICHT—PROPORTIONAL GALVANOMETER.

(Centralblatt, B. V., No. 19, 1883, p. 421.)

Fig. 1 shows the arrangement of the ordinary form of H. Jenkin's proportional galvanometer, in which the magnet is placed at the centre of two coils at right angles to each other, for the measurement of resistances by the differential method. In this arrangement we have

$$\tan.\,\phi = \frac{r + X}{r + R}, \text{ or } X = (r + R)\tan.\,\phi - r \quad \dots \quad \dots \quad (1)$$

In this form the instrument does not lend itself to every-day use, since X is not directly proportional either to ϕ or to tan. ϕ, and the resistance cannot be read off directly.

The author has therefore introduced an improvement by winding on *each* coil several turns of wire belonging to *both* circuits. Let i be the current through the standard resistance R, and i' that through X, and suppose i passes a times round coil I, and then c times round II, whilst i' goes b times round I and *d* times round II. We have to determine the number of turns, on the condition that between the limits X = A and X = A + B, and $\phi = 0^\circ$, $\phi = 90^\circ$, a close proportion shall exist between X — A and ϕ, and that for

$$\left.\begin{array}{ll} X = A & \phi = 0^\circ \\ X = A + \frac{B}{2} & \phi = 45^\circ \\ X = A + B & \phi = 90^\circ \end{array}\right\} \quad \dots \quad \dots \quad \dots \quad (2)$$

Since the magnetic moments of the coils I and II are as

$$a\,i \mp b\,i' : d\,i' \pm c\,i,$$

it follows that for

$$\left.\begin{array}{l} X = A;\ a\,i \pm b\,i' = 0 \\ X = A + \frac{B}{2};\ a\,i \pm b\,i' = d\,i' \pm c\,i \\ X = A + B;\ d\,i' \pm c\,i = 0 \end{array}\right\} \quad \dots \quad \dots \quad (3)$$

Remembering that $i : i' = r + X : r + R$, we obtain the following number of turns:—

a is independent, and may be anything.

$$\left.\begin{array}{l} b = a \, . \, \dfrac{r + A}{r + R} \\ c = a \\ d = a \, . \, \dfrac{r + A + B}{r + R} \end{array}\right\} \quad \dots \quad \dots \quad \dots \quad (4)$$

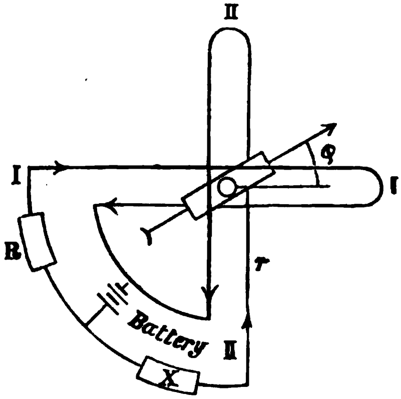


Fig. 1.

The magnetic moments of the rings I and II will then be as

$$a\,i - a\frac{r + A}{r + R}i' : a\frac{r + A + B}{r + R}i' - a\,i \quad \dots \quad \dots \quad (5)$$

The quotient of the two moments is

$$\tan.\,\phi = \frac{X - A}{B + A - X} \text{ or } X - A = \frac{B}{1 + \cot.\,\phi}$$

which satisfies the conditions laid down, since for $X - A = 0, \frac{B}{2}, B$ we have $\tan.\,\phi = 0, 1, \infty$, and therefore $\phi = 0^\circ, 45^\circ, 90^\circ$.

It appears, therefore, that it is only necessary to mark the divisions, not on the circle, but on the chord, to be able to read off the values at once. Or, as shown in Fig. 2, it is easy to determine the divisions of the arc from those of the chord.

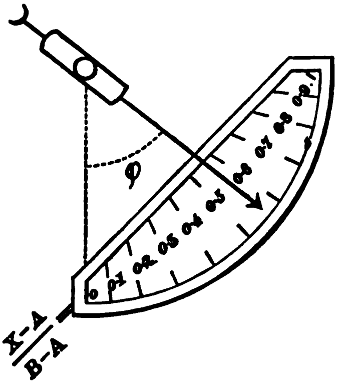


FIG. 2.

In the above calculation several quantities are introduced which in practice might be omitted. For instance, it is desirable to have the number of turns

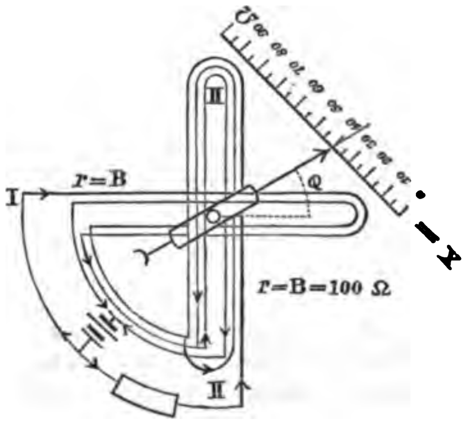


FIG. 3.

on the coils some simple multiple of each other. This can be done either by making the standard B equal to the smallest value of X to be measured, equal to A, and the resistance of each branch $r = B - A$, or we put $A = 0$, $R = 0$, and $r = B$; in this case the magnetic moments of I and II are as

$$a\,i - a\,i' : 2\,a\,i' - a\,i.$$

Such an arrangement of the galvanometer for $A = R = 0$, $r = B$ is shown in Fig. 3

Of five equally long wires, two are wound on I and three on II, each wire making, say, one hundred turns; the ends are then connected up as shown above, and the resistances of both coils with their connections (r) made equal to B, say 100 ohms. With such an instrument resistances from 0 to 100 ohms can be measured. For greater resistances a shunt must be used.

R. HANDMANN—NEW LIQUID FOR BICHROMATE CELLS.

(*Centralblatt für Elektrotechnik*, *B. V.*, *No.* 19, 1883, *pp.* 424-429.)

Bunsen's zinc carbon battery is the most powerful one which up to the present we possess; but its constant use, particularly in rooms, is rendered impossible owing to the fumes given off by it when working. The evolution of the nitrous fumes has been got rid of by using a solution of bichromate of potash as exciting liquid, but at the expense of the duration and efficiency of the cell. Mr. Egger has, as the result of many experiments, arrived at the following as the best composition for the exciting liquid:—

Sulphuric acid	200 cubic centimètres.
Bichromate of potash ...	25 to 50 grammes.
Nitric acid (34 %)	100 cubic centimtères.
Water	200 cubic centimètres.

The following table gives a comparison of the strength of current in the several cases:—The exciting liquid was, in A, pure nitric acid; in B, 100 ccm. sulphuric acid, 1,000 ccm. water, and 80 grammes bichromate of potash; in C, 100 ccm. sulphuric acid, 300 ccm. water, and 60 grammes of bichromate; in D, Egger's solution as above, with 25 grammes of bichromate; and in E the same, but with 50 grammes of bichromate. In columns A, B, and C, the intervals of time between the observations were about half an hour; for D and E two hours.

Observation	A.	B.	C.	D.	E.
1	59·838	50·200	45·150	64·256	46·837
2	57·730	37·850	39·206	64·256	45·180
3	53·814	28·965	35·140	64·256	45·180
4	48·478	18·273	28·965	61 997	45·180
5	...	...	23·393	61·997	45·180
6	...	...	...	59·828	45·180
7	...	...	...	55·722	45·180

The superiority of Egger's liquid is self-evident, the last solution, E, with 50 grammes of bichromate, being especially remarkable for constancy. This cell was kept on closed circuit for 17 hours, with the result that the current at the end of that time was still 90 % of the initial current.

Dr. H. HAMMERL—MEASUREMENT OF THE INTENSITY OF VERY BRIGHT SOURCES OF LIGHT.

(*Elektrotechnische Zeitschrift, B. IV., H.* 6, *June*, 1883, *p.* 262.)

The difficulty experienced in comparing an arc lamp with any unit of light has induced the author to give a practical development to the plan of Dr. Pfaundler. The light coming from the intense source is not allowed to fall directly on the screen of the photometer, but is first reduced mechanically, so that only a certain fraction of the whole light is compared directly with the standard.

To effect this reduction of the light, a blackened circular disc of metal, with sectors cut out of it (Fig. 1), is introduced between the light to be measured and the photometer, and is rotated about a horizontal axis.

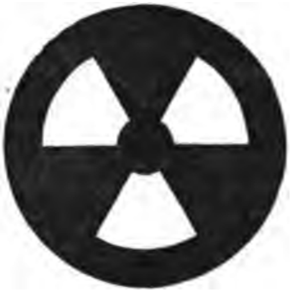


FIG. 1.

The intensity of the light is then reduced in the ratio of the sum of the three angular apertures to 360°. In the case shown in the figure, where the angular aperture of each sector is 60°, the light is reduced in the ratio $\frac{180}{360} = \frac{1}{2}$.

It may be objected that, while to reduce the light from $\frac{1}{2}$ to $\frac{1}{3}$ the sum of the angular apertures is diminished by 50°, the reduction of the light from $\frac{1}{36}$ to $\frac{1}{72}$ allows of a diminution of only 5°. In practice, however, this objection is of no importance, as in most cases it is sufficient to reduce the light to $\frac{1}{20}$ or $\frac{1}{25}$.

It is of course unnecessary to have a number of metal screens with angular apertures of various sizes, as by mounting two discs, as in Fig. 1, parallel to each other on the same axis, the amount of opening of the combined sectors may be made anything we please from 180°, when the openings in both correspond, to 0°, when the openings in one are closed by the solid parts of the other. The author has made a long series of experiments with two such discs, and has found an almost perfect accord between the observed and the calculated values.

Instead of two discs, we may employ only one, with sectors cut out as shown in Fig. 2—*i.e.*, the angular aperture of each sector gradually diminishes from the centre to the circumference, so that each zone corresponds to a different degree of reduction of the light. The disc shown would suffice to give

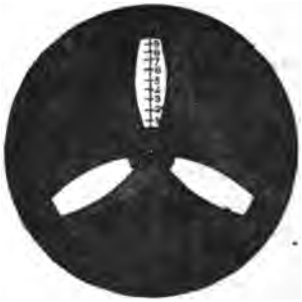


FIG. 2.

all degrees of reduction from $\frac{1}{3}$ to $\frac{1}{30}$, which is amply sufficient for all practical purposes. With such an arrangement it would of course be necessary to use a grease *line*, instead of a grease *spot*, on the photometer screen. In taking a measurement it is then necessary to see at what point in the grease line the passage from dark to bright occurs, and the corresponding line on the sector will give the degree in which the light has been reduced. It is also possible to construct a disc with sectors of such shape and dimensions that, instead of the aperture decreasing by an even number of degrees, as is shown in Fig. 2, in which case the reduction of the light does not proceed by equal decrements, this reduction shall proceed by equal successive steps; thus if, counting from the centre outwards, the apertures have the following values, 120°, 60°, 40°, 30°, 24°, 20°, 17·1°, 15°, 13·3°, 12°, the light will be reduced respectively to $\frac{1}{3}$, $\frac{1}{6}$, $\frac{1}{9}$, $\frac{1}{12}$, $\frac{1}{15}$, $\frac{1}{18}$, $\frac{1}{21}$, $\frac{1}{24}$, $\frac{1}{27}$, $\frac{1}{30}$.

Dr. H. ARON—EFFICIENCY OF ACCUMULATORS.

(*Elektrotechnische Zeitschrift, B. IV., H.* 8, *Aug.*, 1883, *p.* 342.)

The author distinguishes four kinds of efficiency. The efficiency of the charge n is the ratio of the quantity of electricity which passes through the accumulator on discharging to that on charging; that is, $n = \frac{q}{Q}$, where $q = \int_0^t i \cdot dt$ and $Q = \int_0^T I \cdot dt$. If (i) and (I) are the mean values of the current of discharge and charge respectively, we may write $n = \frac{(i) \cdot t}{(I) \cdot T}$. If the deposits a and b in copper or silver voltameter are measured at the same time in charging and discharging, then $n = \frac{b}{a}$.

Secondly, we may consider the efficiency of the chemical action K. This is the ratio of the work given out by chemical processes during the discharge to the work absorbed during charging for the solution of the compounds formed, and for the restoration of the atoms to their primitive state.

$$N = \frac{\int_0^t e \cdot i \cdot dt \cdot}{\int_0^T E \cdot I \cdot dt \cdot}$$

Since e is generally smaller than, and at most equal to E, N may be equal to n, but is generally less.

The electrical efficiency, K, is the third kind. It is the ratio of the work usefully employed during discharge to the whole electrical work absorbed in charging. K differs from N, owing to the influence of the resistance in the element itself, which increases the work necessary for charging as compared with the chemical work, and which during discharge diminishes the useful return of the chemical work.

If R and r are the resistances on charging and discharging respectively,

$$K = \frac{\int_0^t (ei - i^2 r)\, dt}{\int_0^T (EI + I^2 R)\, dt}$$

Hence K is always less than N.

The fourth kind of efficiency is that of the mechanical energy, M. This value differs from K by the factor by which the useful electrical energy of the dynamo machine differs from the work given out by the motor, so that M is less than K. Hence we have always

$$n > N > K > M.$$

In practical work it is the efficiency K with which we are concerned. But K also depends on the internal resistance of the accumulator, and this resistance varies with the number, arrangement, and size of the electrodes, so that for the general question of accumulators or no accumulators, the quantity N is the most important. Now the value of N can be approximately determined.

$$N = \frac{\int_0^t ei\, dt}{\int_0^T EI\, dt}$$

If now we suppose that during the charging E is fairly constant, and that the accumulators are only used for so long that e also remains practically constant, then

$$N = \frac{e\int_0^t i \cdot dt}{E\int_0^T I \cdot dt}$$

or $$N = \frac{e}{E} \cdot \frac{q}{Q},$$

or $$N = \frac{e}{E} \cdot n.$$

If, as was the case at the experiments at the Conservatoire des Arts et Metiers with Faure cells, $e = E$, then $N = n$. Reynier has found that $E = 2{\cdot}2$ volts, and $e = 2$; hence, in this case, $N = 0{\cdot}9\ n$.

The experiments of Hallwachs (see Journal, No. 49, p. 442) may serve to test the truth of the general conclusion that

$$N = 0{\cdot}9 \frac{t \cdot (i)}{T \cdot (I)}$$

T Hours.	I Ampères.	t Minutes.	i Ampères.	$n = \frac{q}{Q}$	N	$\frac{N}{n}$
10	6·9	123	10·1	0·3	0·28	0·93
1	19·4	5	13·2	0·06	0·06	0·99
4	3·9	15	13·0	0·21	0·21	0·99
4	17·1	590	1·8	0·26	0·24	0·92
4	1·1	12	1·7	0·08	0·06	0·87
13	1·0	48	1·6	0·1	0·09	0·90
4	6·9	178	1·6	0·17	0·105	0·88
4	7·2	435	1·7	0·43	0·37	0·86
4	10·8	647	1·7	0·42	0·35	0·83
7	7·4	900	1·7	0·49	0·40	0·82
1	6·7	128	1·6	C·51	0·45	0·88
4	8·3	303	3·8	0·58	0·50	0·86
4	4·0	36	13·5	0·50	0·47	0·94
4	6·0	93	9·4	0·54	0·49	0·91
4	4·0	39	13·2	0·53	0·48	0·91
4	4·2	27	13·2	0·35	0·28	0·80
3	6·1	7	14·7	0·09	0·08	0·89
4	4·1	21	12·6	0·27	0·23	0·85
4	4·0	23	14·3	0·34	0·32	0·94
2·5	21·4	131	6·0	0·24	0·20	0·83
4	3·6	25	14·8	0·43	0·405	0·94
				Mean ...	... ...	0·89

SCRIVANOW'S CHLORIDE OF SILVER CELL.

(*Dingler's Journal*, *B.* 248, 1883, *p.* 178.)

The cell consists of a prism of retort coke, which is covered on all sides with pure chloride of silver. This prism is in a solution of caustic potash or soda (1·30° to 1·45° B.) in 30 to 40 per cent. of water. The attacked electrode consists of a cylinder or plate of zinc, which is brought close up to the carbon prism; in the horizontal form the zinc plate is perforated, and several cells are placed on the top of one another. To prevent a loss of silver the carbon is wrapped in asbestos paper. In the vertical form the carbon rests on a disc of gutta percha placed on the bottom of the zinc cylinder. When the cell is exhausted by the reduction of the chloride the carbon is taken out, well rinsed, and plunged in a bath containing a solution of 100 parts nitric acid, 5 to 6 parts hydrochloric acid, and 30 parts water, by which the reduced silver is reconverted into chloride. The distinguishing feature of this cell is the use of the alkaline liquid.

FILAMENTS FOR MAXIM LAMPS.

(*Dingler's Journal*, *B.* 248, 1883, *p.* 240.)

Instead of carbonising filaments previously brought to the proper shape, Maxim proposes to make filaments as ordinary carbon rods are made. For this purpose he uses a strong vessel, which can be readily heated below, to contain the matter to be carbonised, which is pressed out through the nozzle of the machine by a piston under very considerable pressure. By an ingenious arrangement of crank levers actuated by a revolving ring to which they are attached at one end, the opening of the nozzle can be alternately expanded or contracted, so that the thread of carbon forced out takes the form of a thin filament, with enlargements at regular distances; when cut up these enlargements become the bases of the filament which are fixed to the leading-in wires.

BROWN'S ELECTRIC REGULATOR FOR MARINE ENGINES.

(*Dingler's Journal*, *B.* 248, 1883, *p.* 257.)

The object of this invention is to prevent racing when the propeller is out of water. One contact is fixed at the point which must always remain under water to prevent racing, and a second at a point which is never out of the water, however much the ship pitches. The two contacts are in connection with a dynamo machine driven from the propeller shaft, and when both are under water the circuit is complete and a current flows. As soon as the upper contact is out of water the circuit is interrupted, the armature of an electro-magnet in the circuit is freed, and can be pulled over by a spring. The pulling-over of the armature starts a small auxiliary engine which closes the expansion valve. As soon as the upper contact is covered again by the water, the reverse action takes place. In place of the spring, a second electro-magnet may be used, which is thrown into action by a relay when the first is interrupted.

ELECTRICAL STEERING OF BALLOONS.

(*Dingler's Journal, B.* 248, 1883, *p.* 257.)

G. Tissandier has made some experiments with an elongated balloon of some thousand cubic mètres capacity. The motor is made up of three parts—a propeller with blades of 2·85 mètres diameter, made according to the design of V. Patin, a very small Siemens dynamo, and a bichromate battery. The propeller only weighs 7 kilos; the blades are made of silk, covered with gum-lac, and stretched on steel wires. The helix of the dynamo is very long in proportion to the diameter. The frame is of steel, and the whole machine weighs 55 kilos. The speed of the dynamo is reduced by gearing to $\frac{1}{10}$, and it can give out 100 kilogrammètres per second, with an efficiency of 55 per cent. The current was 45 ampères, and the difference of potential at the terminals 40 volts. The battery consisted of 24 cells connected in series. Each cell was made up of 10 zinc plates and 11 carbon plates in an ebonite vessel containing 4 litres of solution, the whole cell weighing 7 kilos. The battery maintained a constant current for 2½ hours. From the experiments made at Auteuil, it was calculated that the apparatus, with a total weight of 3 men, could do the work of 12 or 15 men for 3 consecutive hours.

E. REYNIER—NOTE ON M. G. TROUVÉ'S DESCRIPTION OF HIS BICHROMATE BATTERY.

(*Comptes Rendus, T.* 96, *No.* 13, 1883, *p.* 838.)

In the first table in his paper M. Trouvé gives the figure 456 grammes as the consumption of zinc for 12 cells in 5 hours, or 7·6 grammes per cell per hour. In this experiment the current at the beginning was 12·6 ampères, diminishing to 6·3 ampères, or an average of 9·45 ampères. Now, on theoretical grounds, the consumption of zinc per cell per hour for 9·45 ampères should be 11·34 grammes; which throws some doubt on the correctness of M. Trouvé's experiments.

TRESCA—M. LEMSTRÖM'S OBSERVATIONS IN LAPLAND.

(*Comptes Rendus, T.* 96, *No.* 19, 1883, *p.* 1335.)

On the top of a mountain was placed a metallic conductor furnished with a great number of vertical points all in connection, and joined through a galvanometer to a zinc plate buried in damp earth several hundred metres below the level of the conductor. With this apparatus M. Lemström has proved that in these high latitudes, and at a temperature of −20° C., the aurora borealis is essentially an electrical phenomenon, to which correspond currents in the atmosphere, continued in the apparatus, and comparable with the current which would be produced by a Leclanché cell of medium size. The natural manifestation of this current gives rise, even in the absence of all other light, to a local aurora which is apparent above the pointed conductor.

and in which the wave $\lambda = 5{,}569$, characteristic of all such phenomena, can readily be observed.

It is also possible to recognise with entire certainty the existence and even the value of the electric forces which are brought into action under these circumstances, which can be started and interrupted at pleasure.

These experiments were made on two very high points, the Orantunturi and the Pietarintunturi, with apparatus covering 900 square mètres of surface, and there is reason to believe, as a first approximation, that the current thus produced, which is proportional to the surface covered by the pointed conductor, varies with the latitude and the season of the year. It is not surprising that very exact values were not obtainable, when we consider that the conducting wires were in a few minutes covered with so great a quantity of ice as to break under its weight.

M. Lemström proposes to continue his observations this winter, and to attempt a solution of the following questions:—

In what manner should the apparatus be arranged so as to give for a fixed extent of surface a maximum current?

What is the ratio between the current and the extent of surface covered?

In what way does the current vary with the latitude and with the difference in elevation between the metallic points and the earth plate?

What effect have the seasons?

What relation exists between the currents in the air, the current in the earth, and the magnetic variations?

C. RESIO—ELECTRIC DYNAMOGRAPH, OR RECORDER OF WORK DONE BY MACHINES.

(*Comptes Rendus, V.* 96, *No.* 19, 1883, *p.* 1361; *and La Lumière Electrique, V.* 9, *No.* 20, 1883, *p* 81.)

The transmitting part of the dynamograph consists of a simple mechanical arrangement attached to the driving shaft, by means of which the torsion of the shaft causes a ring surrounding the shaft to slide in the direction of its length, so that its movements are proportional to the torsion. A metal rod of rectangular section is fixed by the side of the ring, parallel to the shaft, and receives, between two small jaws, the flat edge of the ring. The rod therefore follows all the movements of the ring, and its motion is also proportional to the torsion of the shaft. The movements of an index fixed on the rod are transmitted electrically to the receiver instrument, which traces a curve, the ordinates of which are proportional to the torsion of the shaft, and the abscissæ to its angular velocity. The mechanical arrangement consists of a clock which each minute sends two currents into a circuit, in such manner that the time which elapses between two consecutive emissions—which is always less than sixty seconds—is exactly proportional to the displacement of the index, and therefore to the torsion. The part of the receiver which registers the torsion consists also of a clock which gives a uniform motion of rotation of one turn per minute to a cylinder having on its surface a spiral line which is kept covered with ink. The spindle of this cylinder moves it by friction only, so that the cylinder can be stopped while the spindle still continues to revolve. The effect of the first current sent by the transmitter is to release the cylinder, and therefore to allow it to start rotating; the second current circulates through the coils of an electro-magnet, which, when thus excited, brings in contact with the spiral on the cylinder a paper band, which is drawn along at a speed proportional to that of the machine, and which is thus marked by the inked spiral. A similar mark is made on the paper by the first current. The distance between the two marks is proportional to the time which elapses between the two currents, and hence to the torsion of the shaft—*i.e.*, to the power transmitted by it. The first mark is made at the moment of release of the cylinder, and, as the paper moves slowly, and these currents succeed each other rapidly, the marks run into each other, so as to form a straight line parallel to the edge of the paper band. The second series of marks form the curve mentioned above, from which the power transmitted can be calculated.

MARTIN DE BRETTES—AUTOMATIC PRINTING OF MESSAGES TRANSMITTED BY LIGHT.

(*Comptes Rendus*, *V.* 96, *No.* 26, 1883, *p.* 1856.)

The rays from an electric arc, rendered parallel by a Mangin projector, would be sent from the one station, and would be received at the other on a converging lens, in the focus of which is a selenium cell. This cell is in circuit with a local battery and the receiving instrument, which would be so adjusted, as regards tension of spring, resistance, etc., that the changes of resistance produced in the selenium cell by the action of the light, would alter the amount of current passing, and so act on the electro-magnet of the receiver. If an ordinary ink-writer is used, the Morse alphabet can be recorded; or by using one of Bréguet's instruments in which the needle is replaced by a type-wheel, the messages can be printed directly. The distance over which such a telegraph could work could only be determined by experience, as we do not know the law governing the decrease of light in this case. However, Fizeau, in his celebrated experiments, using only an ordinary oil lamp, was able distinctly to see the light at a distance of about ten miles and a half.

G. **CABANELLAS**—DETERMINATION OF THE INTERNAL RESISTANCE OF ANY ELECTRIC SYSTEM WHATEVER, IRRESPECTIVE OF THE DISTURBING E.M.F. IN IT.

(*Comptes Rendus*, *T.* 97, *No.* 5, 1883, *p.* 311.)

The author lays down this theorem:—If any electric system whatsoever, in a permanent condition of potentials, is joined by any two points in it to any second electric system, it is possible, without changing any of the actions of the first system on the second, between the two points to reduce the first system to the simple expression of a resistance equal to the inert resistance of the system between these two points, and to an E.M.F. equal to the difference of the original potentials of the two points of the first system.

The actual measurement is made in the following way:—

Let R be the inert resistance of the system between the points A and B at potentials V_A and V_B measured on open circuit, and let $V_A - V_B = e$. Close the circuit through an external resistance, r, in which there is an E.M.F., E, opposed to e. Call R_E the apparent resistance, measured directly either by the bridge or by two galvanometers, whilst the internal force, e, is acting. If E and i are the E.M.F. and current during the measurement we shall have $R_E = \frac{E}{i}$, but $E = e + Ri$, then $R_E = R + \frac{e}{i}$; now $i = \frac{E - e}{r + R}$, therefore

$$R_E = R + \frac{e\,(r + R)}{E - e}$$

We see at once that R_E can only equal R if $e = o$, or if $E = \propto$; for any finite value of e, R_E will vary from $\propto$ to R, as E varies from e to $\propto$.

Put now $n = \frac{e}{E}$, and the above formula becomes

$$R_E = R + \frac{r + R}{n - 1},$$

whence

$$n = 1 + \frac{r \div R}{R_E - R}$$

A second determination with r' instead of r will give

$$n' = 1 + \frac{r' + R}{R_{E'} - R}$$

If we divide one equation by the other, remarking that $\frac{n}{n'} = \frac{E}{E'}$, we have

$$\frac{E}{E'} = \frac{1 + \frac{r + R}{R_E - R}}{1 + \frac{r' + R}{R_{E'} - R}},$$

from which we find

$$R = \frac{R_E \cdot E(R_E + r') - R_{E'} \cdot E'(R_E + r)}{E(R_{E'} + r') - E'(R_E + r)}$$

L. LOSSIER—INDIRECT MEASUREMENTS OF VERY SMALL RESISTANCES.

(*L'Electricien, Vol. VI., No. 55,* 1883, *p.* 57.)

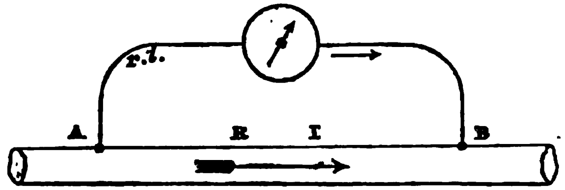
Suppose it is required to measure the resistance, R, of a large conductor between the points, A and B, this conductor being traversed by a current of strength, I.

A galvanometer graduated very exactly in fractions of an ampère is joined up as a shunt between A and B, the resistance r being known.

From Kirchhoff's law we have, then, $R\,I - r\,i = O$, whence

$$R = r \cdot \frac{i}{I},$$

which reduces the measurement to that of two currents. If we wish to



measure the resistance of a machine at work, since we have an E.M.F. in the circuit, we must put $R\,I + r\,i = E$, whence.

$$R = \frac{E - r\,i}{I}.$$

Example 1.—Large conductor of 25 sq. cm. section.

$I = 937$ ampères, $i = 0{\cdot}00145$ ampère.

$r = 100$ ohms; $R = 0{\cdot}000155$.

Example 2.—Large electro-plating machine, capable of working up to 5,000 ampères.

$E = 2\,.\,1$ volts, $I = 1{,}150$ ampères, $i = 0{\cdot}011$ ampères.

$r = 100$ ohms; whence $R = 0{\cdot}00087$.

The above measurements are some actually made by the author, who, it may be added, claims nothing new in his method.

E. BOISTEL—WHEATSTONE BRIDGE FOR MEASUREMENT OF VERY SMALL RESISTANCES.

(*L'Electricien, Vol. VI., No. 54, 1st July,* 1883, *p.* 20.)

The accompanying diagram will almost suffice to make clear the plan adopted.

The rod or wire, the resistance of which is to be measured, rests in two forks formed by four blocks of brass, F_1, etc., of the form shown in section below the diagram. Two of these brass blocks are joined up to the resistances R_2 and R_4; the third, F_1, is connected with the battery, and the fourth, F_2, with the stretched wire, A B, of German silver. This wire is fixed by the side of a graduated scale, and is provided with two sliding contacts, *a* and *b*, which are respectively in connection with the resistances R_3 and R_1.

It is evident that equilibrium will be established, shown by no deflection on the galvanometer, when

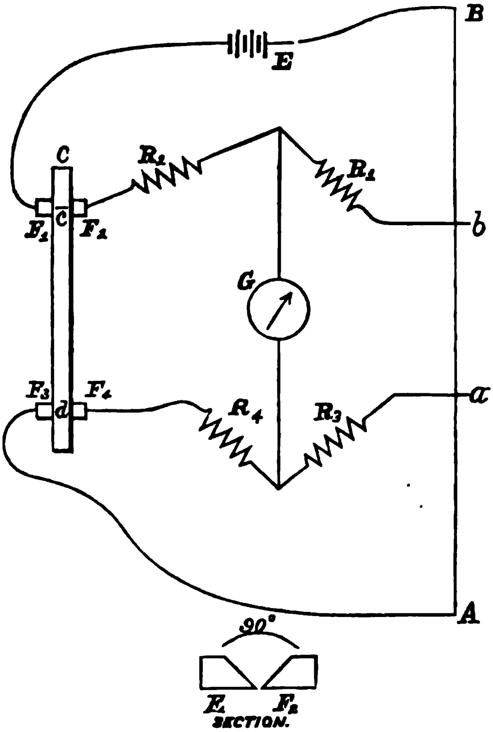
$$\frac{R_1}{R_2} = \frac{R_3}{R_4} = \frac{R\,ab}{R\,cd}.$$

$R\,cd$ is the resistance of the bar between the points *c* and *d* where it rests on the blocks F. Hence

$$R\,cd = R\,ab\,\frac{R_2}{R_1} = R\,ab\,\frac{R_4}{R_3}.$$

For the measurement of resistances between 0·01 ohm and 0·0001 ohm, each of the resistances R_1, etc., is made equal to 10 ohms; for still smaller resistances up to 0·000001 ohm, R_1 and R_3 are made 1,000 ohms, and R_2 and

R_4 remain 10 ohms. It will be seen that the resistance measured is independent of the good or bad contact of the bar with the brass blocks.



As an example, suppose we have a copper rod of 487 square millimètres section, and that *cd* is 80 centimètres, then making R_1 and R_3 each 1,000 ohms, and R_2 and R_4 each 10 ohms, if the resistance of the German silver wire is 0·0000208 ohm per millimètre, and the sliders *a* and *b* stand at 65 and 10 millimètres, $R_{cd} = 55 \times 0{\cdot}0000208 \times \frac{1}{100} = 0{\cdot}0000115$ ohm.

DU MONCEL.—SPEED REGULATORS FOR ELECTRICAL INSTRUMENTS OF PRECISION.

(*La Lumière Electrique, Vol. VIII., pp.* 460 *and* 497 ; *Vol. IX., pp.* 33, 65, *and* 97 ; *Nos.* 15, 16, 19, 20, *and* 21 ; 1883.)

The author has devoted five articles to the description of the most noteworthy forms of governors for electrical apparatus generally, which he has divided into two classes—the electrical and the purely mechanical governors.

Among the former, one of the most important, and the one which lends itself to the greatest number of applications, is the phonic wheel of M. Paul Lacour. In its simplest form the phonic wheel consists of a soft-iron solid circular disc, provided with teeth on its periphery; the teeth revolve just in front of the core of a straight electro-magnet; the whole may therefore be looked upon as an elementary electro-motor. If the wheel be once started, and a succession of currents be sent through the electro-magnet at regular intervals, the motion will be kept up, and the speed will remain constant when the wheel, during the period of each current, revolves through a distance equal to that between two consecutive teeth. In order to send the successive currents a tuning fork is used, the prongs of which vibrate between the poles of an electro-magnet, and alternately make and break the circuit of the battery working the phonic wheel, the vibrations being kept up by the action of the electro-magnet. The angular velocity depends not only on the rate of vibration of the tuning fork, but also on the number of teeth of the wheel; it is, however, more difficult to work with wheels with few teeth. With a tuning fork giving 245 vibrations per second, and a wheel with 18 teeth 21 mm. apart, M. Lacour obtained a speed of 245 × 21 mm. = 5·145 metres per second, or, say, 20 kilometres per hour. In order to do away with any slight variations in speed, a small circular wooden box may be fixed on the top of the phonic wheel, and filled with mercury, which will not only add to the moment of inertia, but will itself tend to move on if the wheel goes slower, and will tend to lag behind if the wheel goes quicker.

The applications which may be made of the phonic wheel are very numerous. It may be employed as a chronograph, or as a clock; in the latter case the vibrator may be in a place sheltered from all disturbing influences, and the wheel may be placed where required; or several wheels may be connected up on one circuit, so that several clocks in different places may all be made to go together exactly. Again, it may replace the siren in the determination of the pitch of a sound; or it may be used as a speed counter. One of its most important applications is to the multiple telegraphs of Meyer and Baudot, or to the Hughes printing instrument, in which absolute synchronism is essential. The following method of determining the speed of rotation of a machine shows another application. The phonic wheel has cut out of it a number of small holes arranged in concentric circles; say ten circles with five holes in the innermost one, ten in the next, and fifty holes in the tenth circle. When the wheel revolves above a white surface it will appear as though covered with alternate black and grey rings. Attached to the machine, the speed of which is to be measured, is a cylinder with black and white stripes parallel to its axis, so that it shows a grey tint on revolving; this cylinder is placed below the phonic wheel. If the machine has a speed which will make the cylinder advance through 35 stripes whilst the wheel makes one revolution, the seventh ring will no longer be all grey, but will show alternate black and grey stripes. If the speed increases, these stripes will begin to move in the same direction as the cylinder, and finally will appear in the eighth ring, showing that the machine is then running at such a speed that the cylinder advances through forty of its stripes for each revolution of the phonic wheel.

The second electric regulator is that of Marcel Deprez. It is extremely simple, but spasmodic in its action, consisting of a pliant tongue of steel fixed at one end to, and parallel with, some rotating axle of the machinery; the other end is weighted and provided with a set screw, which makes contact with the axle when the speed is not sufficient for the centrifugal force to overcome the elasticity of the tongue; as soon as the speed becomes too high, however, the tongue moves away from the axle, and contact is interrupted.

In the system of M. E. Mouline, a centrifugal governor actuates a commutator, which closes the circuit of one or other of two magnets; these, by means of levers, act on the stop-valve of the steam-engine to which this kind of governor is more specially applicable.

M. Courtin has gone beyond the simple centrifugal governor, and has invented a so-called "parabolic governor." A sort of trough is bent into a parabolic form in a vertical plane, and rotates with the centrifugal governor. At the bottom of this trough, and on only one leg of the parabola, are fixed two metal strips insulated from each other; the one is continuous, but the other is interrupted by several pieces of ebonite. The two strips form two rails on which rolls a spindle-shaped piece of metal, formed by placing two cones base to base. The two strips are in an electric circuit, and accordingly, as the double cone rolls more or less up the leg of the parabola, a greater or less number of contacts will be made between the continuous rail and the interrupted one. These closings of the circuit can be made use of to work any electro-magnetic arrangement for controlling the stop-valve.

The author notices briefly the several kinds of governors which have been brought into use by various inventors to regulate the speed of telegraph apparatus, and then passes on to examine the purely mechanical governors, and describes in the first place Watt's ordinary centrifugal regulator, to which Foucault subsequently added a counterpoise to increase the efficiency. M. Villarceau, in his governor, has combined the centrifugal action of heavy balls with the retarding action of two or more vanes. The vanes, when at rest, hang straight down, being pivotted at one end to a crossbar attached to the vertical spindle of the governor. Two weights slide on two rods fixed, one on either side, into the head of the vane near the pivot, these rods being in the same plane as the vane; as the speed increases, the vanes tend to fly out more and more, and experience increasing resistance from the air. To each value of the motive force corresponds only one position of stable equilibrium of the system of vanes and weights, and it is, of course, easy to arrange that this position is reached just at the speed required.

Lastly the author describes very fully the form of regulator which has been named "Elliptic" by its inventor, M. van Rysselberghe, who requires a very accurate form of governor for his meteorograph. This apparatus depends on the principle that, in order to obtain a rigorous isochronism of a motor, it is necessary that the regulating weight should be kept on a parabola, and that the parabola is the limit towards which an ellipse tends when its semi-major axis a is indefinitely increased, whilst the parameter $\frac{b^2}{a} = p$ remains constant. Hence a very close approximation to absolute isochronism can be obtained by keeping the governing weight in a very elongated ellipse. In its simplest form this governor consists of four arms jointed together so as to form a diamond shape; the upper point of the diamond is fixed to the vertical axle of the governor, while the lower end is jointed to a sleeve which can slide up and down the axle; on each of the lower arms, near their point of junction, is fixed a weight, and these weights will describe part of an elliptical curve, which can be shown to differ very slightly from a parabola.

O. KERN—THE FIRST ELECTRIC MOTOR.

(*La Lumière Electrique, Vol. IX., No.* 19, 12*th May*, 1883, *p.* 40.)

In the Italian section of the Paris Exhibition of 1881, was a very interesting document giving the particulars of two forms of motor constructed by the Abbé Salvator dal Negro, Professor of Natural Philosophy at the University of Padua, in 1830 and 1831 respectively.

The former consisted of a compound bar-magnet suspended vertically, at about two-thirds of its length from the upper end, on two pivots which rested on a support, the lower end having a counterpoise attached. The upper end of the magnet oscillated between the two poles of a fixed horse-shoe electro-magnet, the wires of which were led to a rocking commutator which reversed the direction of the current at each half-oscillation of the pendulum-magnet. The result of this arrangement was that when the magnet had been attracted to one of the poles of the electro-magnet the movement reversed the connections, and repulsion took place.

In the later form, the compound bar-magnet was arranged as a horizontal lever oscillating in a vertical plane about a fixed fulcrum. One end was provided with an armature, below which were placed the poles of a vertical electro-magnet. The reversal of the current in the coils on the latter was effected in the same way as in the first motor. To the other end of the permanent magnet were fixed two vertical connecting rods provided with pawls at their lower ends, which engaged with a ratchet-wheel, thus imparting continuous circular motion to a sort of fly-wheel, which consisted of the spokes only, loaded at their extremities with heavy knobs.

The original description of these motors was given in the *Bulletin de l'Academie des Sciences, Lettres, et Arts*, of Padua, Vol. IV., and later, in April, 1834, in the "*Annales du Royaume Lombard-Venetien.*"

Dr. E. LECHER—PHOTOMETRY.

(*Internationale Zeitschrift für die Ausstellung in Wien*, 1883, *No.* 1, *p.* 14.)

The author is of opinion that it is not sufficient merely to compare the luminosity of the light with some standard, but that it should be dissected up, so as to show how much of the energy appears as visible rays and how much is wasted, so far as illumination is concerned, as invisible heat rays. He suggests that some such method of research might be with advantage employed as Professor Langley has carried out with his bolometer in investigating the solar radiation. The results obtained could be plotted down as a curve, and this would at once indicate how much of the radiant energy was really useful as light. For instance, Langley has found 34 per cent. of the sun's total radiation is visible as rays of light, while in an argand gas burner the proportion is only 2·4 per cent. A careful study of various substances would lead us to know which bodies give out the greatest proportion of light energy—a point of some importance.

Dr. H, KRÜSS—PHOTOMETRY.

(*Internationale Zeitschrift für die Elektrische Ausstellung in Wien*, 1883, *No.* 4, *p.* 60.)

The author does not think that it will be practically possible to follow the plan proposed by Dr. Lecher (see abstract above), though something may be done in this direction, and has indeed already been done by comparing two parts of the two spectra of the electric light and of the standard, as, for instance, the red and the green rays. He remains of opinion that it will always be necessary to make a comparison between the total light given out by the two sources. He quotes some results obtained by Dr. F. Fischer of the quantity of heat given off by various sources of light, which are interesting:—

Electric arc light	57 — 158	calories.
„ glow light	290 — 536	„
Regenerative gas flame	1,500	„
Argand gas flame	4,800	„
Petroleum ring burner	3,360	„
„ flat burner	7,200	„
Candles, various	7,960 — 9,700	„

These figures are all calculated for a uniform light of one hundred candle-power, and for one hour's burning.

It has continually been stated that the electric light is more trying to the eye than sun light; yet, in point of fact, the former contains more red and less violet rays than the latter.

One of the great difficulties in photometric measurements of the electric light is that we generally try to compare two lights of different colours. The brightness of any light—*i.e.*, the intensity of the sensation produced in the eye—is a function of the kinetic energy of the vibrations of the æther; the nature of this function is not known, but Helmholz has shown (*Physiol. Optik*, *p.* 21) that it is different for light of different wave-lengths. It may be shown experimentally that two differently-coloured sources of light, which we judge to be equally bright, no longer appear so if we increase or decrease the intensity of both in the same ratio, as has been done by Purkinje. Two differently-coloured sources of light are therefore incommensurable by our eyes, so far as their brightness is concerned. A comparison of the several parts of the spectra will not reach the desired end, since the sums of the intensities of the several parts will not give us the ratio of the whole light from the two sources. The curves obtained by Langley do not give the quantity of light radiated, but the quantity of heat, which is not the same thing.

DR. E. LECHER—PHOTOMETRY.

(*Internationale Zeitschrift für die Elektrotechnische Ausstellung in Wien*, 1883, *No.* 5, *p.* 78.)

The author, in his reply to Dr. Krüss (see abstract above), argues that in comparing two sources of light—*e.g.*, two sodium flames—by means of a Bunsen photometer, we move the two lights to such distances from the screen that the effect of one on the retina of the eye is equal to that of the other, and since the two effects are equal, therefore the causes are equal, these causes being the vibrations of the particles of the æther. We therefore may be said rather to compare two lights physiologically than photometrically, since we compare, not the lights themselves, but our own perceptions. If we use two differently-coloured sources of light, and compare their spectra by means of a bolometer, we can determine the ratio of their intensities; if then we place them at corresponding distances on each side of a Bunsen photometer, they will no longer agree. All that we can hope for is an approximation, and with the present methods we cannot arrive at a really accurate determination.

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